The Behavioral Foundations of Default Effects: Theory and Evidence from Medicare Part D*

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Abstract

We show in two natural experiments that default rules in Medicare Part D have large, persistent effects on enrollment and drug utilization of low-income beneficiaries. The implications of this phenomenon for welfare and optimal policy depend on the sensitivity of passivity to the value of the default option. Using random assignment to default options, we show that beneficiary passivity is extremely insensitive, even when enrolling in the default option would result in substantial drug consumption losses. A third natural experiment suggests that variation in active choice is driven by random transitory shocks rather than the inherent attentiveness of some beneficiaries.

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Across a variety of domains, default rules, which define how a consumer is treated if they fail to make an active choice, appear to have large effects on individual behavior. Opt-out default rules increase enrollment in retirement savings programs (Madrian and Shea 2001, Choi et al. 2002), organ donation (Abadie and Gay 2006), and electricity pricing schemes (Fowlie et al. 2021) relative to opt-in rules. Default rules are one of the few "nudges" that have been shown to have large effects on consumer choices across many domains. Consequently, today they are widely used to influence consumer behavior.

However, despite the robust evidence for the existence of default effects, there remain two important unresolved questions. The first is whether default rules have effects on real outcomes in high-stakes settings. Recent work in the retirement savings literature has, for example, questioned whether large default effects on retirement savings plan *enrollment* result in equivalently large effects on *actual savings* (Choukmane 2021, Beshears et al. 2021a, 2022). The second is why consumers follow defaults in the first place. The prior literature has offered competing models (Bernheim and Taubinsky 2018), but there exists little evidence as to what specific mechanisms drive observed behavior. Different *positive* models of behavior result in starkly different *normative* conclusions about welfare and optimal policy (Bernheim et al. 2015, Goldin and Reck 2022), making their distinction important for economic analysis.

In this paper, we study default effects in the context of the Medicare Part D Prescription Drug Insurance Program in the United States, one of the largest regulated health insurance markets in the world. Prior work has documented patterns *consistent* with large default effects in this setting, but also consistent with other models of behavior (Abaluck and Gruber 2011, Polyakova 2016). We focus on the Low-Income Subsidy (LIS) segment of the program, which provides subsidized coverage to low-income program beneficiaries. The LIS program has unusual default rules that generate multiple natural experiments: In some circumstances, the default is for beneficiaries to remain in their existing prescription drug insurance plans; whereas in others, the default is auto-assignment to a randomly-chosen plan from a set of qualified plans that are free for beneficiaries to enroll in. Defaults are extremely important in this context: In any given year, *two-thirds* of all LIS beneficiaries are enrolled in a plan that they were originally enrolled in by default (random) assignment. We use this setting to: **(1)** cleanly estimate the effect of default rules on enrollment and drug consumption; and **(2)** test the implications of models of why those effects arise.

To estimate the effect of default rules on enrollment, we leverage two natural experiments. In the first natural experiment, we exploit the fact that the default plan for a *new* Medicare beneficiary in the LIS program (i.e., one who enrolls when they turn 65) is randomly selected from a set of eligible plans. For these beneficiaries, we can directly measure whether they followed the randomly-assigned default. Only 16% of new LIS beneficiaries initially opted out of their randomly-selected default plan and actively chose a different plan, while the remaining 84% were automatically enrolled in the randomly-selected plan. Only 45% of new beneficiaries *ever* made an active choice to opt out of their default after five years. Even beneficiaries for whom the potential value from opting out appears to be the highest rarely do so.

For the second natural experiment, we leverage an exogenous change in the default faced by LIS beneficiaries. Each year, Part D plans submit their yearly premium, after which the premium subsidy for LIS beneficiaries is determined. For beneficiaries enrolled in benchmark plans in year $t-1$ whose plans' year t premium bid is *below* the t subsidy level, their default is to remain in their incumbent plan. However, for beneficiaries previously enrolled in plans whose premium bid falls *above* the subsidy, the default is to be

re-assigned to a different, randomly-chosen plan. This enables a regression discontinuity design, comparing the plan switching rate of beneficiaries whose incumbent plan's bid was just below the subsidy to those whose bid was just above the subsidy. We estimate that the change in default rules from re-enrollment to reassignment raises the rate of plan switching from close to 0% to 97%, almost entirely driven by passive reassignment. Beneficiaries do not appear willing to pay as little as $2 per month to remain in their previous plans. The impact of the default change is long-lasting: Approximately 99% of those who do not initially opt out of the default of reassignment remain in their new plan for at least two years post-reassignment.

Next, we show that different default rules have consequences for real beneficiary consumption outcomes. We use a difference-in-differences design, comparing changes in drug utilization for beneficiaries whose incumbent plans bid just above the LIS subsidy (changing their default to random reassignment) to those whose plans bid just below (maintaining the default of remaining in the incumbent plan) before versus after the bid. Beneficiaries with defaults of reassignment experienced a 6.4% average reduction in drug spending after their default changed compared to those whose defaults did not change. This represents a nontrivial loss in drug consumption, although translating it into welfare terms is challenging without knowing beneficiaries' valuations for the drugs they take. These declines are pronounced for high-value drugs used to treat chronic conditions, and are fairly robust to price standardizations that impose increasingly conservative assumptions about the consumption loss of substituting to alternative drugs.

Having shown that default rules matter for both enrollment and utilization, we then try to disentangle the mechanisms for why beneficiaries follow defaults. Since the model space is large, we cannot test every potential psychological explanation even with our rich data. We focus on a distinction raised by Handel and Schwartzstein (2018) between 'frictional' models, where agents must overcome a costly hurdle in order to exert attention and make an active choice (and thus will only make an active choice when the payoff to doing so is high enough), and 'mental gap' models, where attention is driven by norms, memory, or other factors unrelated to the payoffs of the decision. Workhorse parametric models of default effects (Choi et al. 2003, Carroll et al. 2009, Handel 2013, DellaVigna 2018), tend to be 'frictional' in that they specify an additive wedge in utility between passivity and making an active choice. A testable implication of the frictional class of models is that, under those models, agents' active choice propensity should be elastic with respect to the value of following the default. This elasticity is a key input into welfare and optimal policy considerations, including whether the potential losses from passivity are bounded, and whether optimal default design should aim for paternalism (Thaler and Sunstein 2003, Handel and Kolstad 2015a) or aim for maximizing active choice (Carroll et al. 2009, Bernheim et al. 2015, Goldin and Reck 2022).

Structural approaches typically allow this elasticity to be fully determined by other parameters in the model. However, given exogenous variation in the value of the default option, we can directly estimate this elasticity without additional parametric structure. We do so by leveraging the fact that default re-assignment in our setting is to a plan that is chosen at random. We use the variation that arises from beneficiaries potentially being assigned to a plan that does not 'fit' them well, in that the plan covers a small share of the drugs the beneficiary took in the prior year. We stratify our prior difference-in-differences approach by the fit of the assigned default plan. Beneficiaries whose assigned default was one of the worst-fitting plans available to them reduced their drug consumption by 12.6%, compared to 4.3% for beneficiaries whose default covered more of their prior drugs. When we restrict to beneficiaries who faced larger differences in

'fit' across plans, we find that beneficiaries with the worst-fitting defaults faced drug consumption losses of up to nearly 30%.

If default effects are frictional, we should expect this variation in potential consumption outcomes to result in variation in the propensity to make an active choice (high elasticity of active choice to the value of the default). Instead, we find little difference: Beneficiaries assigned to their worst-fitting plans are only 2.3 percentage points more likely to make an active choice relative to those assigned to their best-fitting plans, even though these unlucky beneficiaries face drug consumption losses that are *three times* as large. We find comparable active choice responses even for beneficiaries who faced large across-plan differences, whose potential drug consumption losses were as high as 30%. Across a wide range of defaults, with consumption effects ranging from around 0 to as high as a 30% reduction, fewer than 10% of beneficiaries ever opt out of their default plan assignment. These results persist for at least two years following the default change.

Rationalizing these results with a 'frictional' model requires that the size of the friction is heterogenous across the population and drawn from a highly lumpy distribution. In a rational inattention model where beneficiaries value drug consumption dollar-for-dollar (a lower bound for value if there is no moral hazard), our results imply that 3.4% of beneficiaries face no frictions, 92.4% of beneficiaries face frictions greater than $1,040, and 4.2% of beneficiaries face frictions somewhere in between $0 and $1,040. We find such a distribution implausible but cannot rule it out per se.

We thus construct an additional auxiliary test of frictional models' explanatory power. A frictional explanation of our above results would posit that the possible variation in incentives induced by different plan assignment is simply insufficient to get more than 4.3% beneficiaries to make an active choice. But in a frictional world, we should expect to see similar magnitudes within-beneficiary, across time: Only 4.3% of beneficiaries should be 'sometimes choosers,' who make an active choice when they face some defaults but not others. We test this using a third natural experiment: All beneficiaries whose plans exit the market face a default of random auto-assignment, giving us a sequence of potential active choices for these beneficiaries. In contrast to the frictional model prediction, 26.5% of beneficiaries are 'sometimes choosers.' A simple calibrated structural model suggests that one-third of the variation in latent attention is driven by across-beneficiary permanent heterogeneity, with the remaining two-thirds driven by transitory shocks to attention within beneficiaries across time.

We conclude from the sum of our results that default rule design has important consequences for beneficiaries. However, rather than being driven by persistent frictions, our results suggest that default effects are driven by transitory positive shocks to beneficiaries' attentiveness or information that is unrelated to the material consequences of their potential choices. Under such an explanation, optimal policy paternalistically guides beneficiaries towards their best options, rather than providing incentives to overcome cognitive hurdles, and beneficiaries may experience large and long-lived welfare losses in the absence of such policy.

Our results are important for three distinct literatures. First, we clarify a robust literature documenting persistence in health insurance choice (Abaluck and Gruber 2011, Handel 2013, Ericson 2014, Polyakova 2016). While, unlike this literature, losses in our setting are not easily measurable in dollars, our setting allows us to distinguish default effects and inattention from switching costs in a novel way.[1] Our results

[1] Heiss et al. (2021) and Drake et al. (2022) make this distinction as well, but their approaches rely on assumptions about certain environmental features being attention-relevant but not choice-relevant.

suggest that the tendency of beneficiaries to stay in their plans over time is explained by default rules rather than beneficiaries not wanting to switch out of their plans. Moreover, we are able to explain why estimated choice frictions in this literature have been implausibly large.[2] In prior research, switching frictions are estimated by measuring how much value agents intentionally forgo by avoiding making an active choice; our finding that beneficiaries are insensitive to this forgone value resolves this puzzle.

Second, we extend the general literature on default rules and their consequences. The literature has generally focused on settings where defaults have relatively small stakes for the agent (tipping, charitable giving) or where the ramifications of following the default are difficult to assess in the short run (retirement savings, vaccination). In contrast, we show that default rules are still powerful when the consequences of passivity are large and realized immediately, and even when this mistake can be rectified quickly ex post. In contrast to recent work on retirement savings (Choi et al. 2004, Choukmane 2021, Beshears et al. 2021a, 2022), we find meaningful effects of default assignments on consumption. Additionally, we are among a small set of recent papers to try to explicitly distinguish between models of passivity (Blumenstock et al. 2018, Andersen et al. 2020), which we extend by considering ex post consequences for consumption rather than ex ante mismatch. Our rejection of 'frictional' models is important given that such models are the norm in research where a model is explicitly specified,[3] and therefore that work may lead to incorrect inferences about welfare.

Finally, our results contribute to a nascent literature testing models of endogenous attention. Recent work in lab and survey settings has found that agents pay more attention to decisions when incentivized to do so (Caplin et al. 2020, Bronchetti et al. forthcoming, Morrison and Taubinsky forthcoming). These papers find large elasticities of attention to the stakes of the decision. In contrast to this work, we find that attention in our setting is surprisingly *in*sensitive to stakes. Our results may more closely reflect real-world attention elasticities, compared to experimental settings which inherently focus the subject's attention.[4]

1 Institutional Setting and Data

1.1 Medicare Part D and the Low Income Subsidy Program

Medicare Part D is the outpatient prescription drug insurance benefit for the elderly in the U.S., introduced in 2006. Drug coverage is provided by private insurers offering insurance plans on a centralized and regulated market; beneficiaries choose from plans offered in one of 34 service regions, based on their geographic location. Part D plans must cover at least two drugs in each of 148 therapeutic classes. Plans are otherwise given some scope for differentiation, in terms of the specific drugs that they cover, as well as drug-specific

[2] E.g. Handel (2013) estimates average yearly switching costs of $2,032, a substantial share of insurance value in his setting.

[3] In health insurance, Samuelson and Zeckhauser (1988), Abaluck and Gruber (2011), Handel (2013), Heiss et al. (2013, 2021), Ericson (2014), Handel and Kolstad (2015b), Polyakova (2016), Ho et al. (2017), Abaluck and Adams-Prassl (2021), Saltzman et al. (2021), Drake et al. (2022). In retirement savings, Choi et al. (2003), Carroll et al. (2009), Bernheim et al. (2015), DellaVigna (2018), Choukmane (2021), Goldin and Reck (2022). Other settings include organ donation (Abadie and Gay 2006), health club membership (DellaVigna and Malmendier 2006), tipping (Haggag and Paci 2014), auto insurance (Honka 2014), charitable donations (Altmann et al. 2019), and electricity plans (Fowlie et al. 2021).

[4] Our paper also shares similarities with recent non-experimental research on attention elasticities in prescription drug delivery (Beshears et al. 2021b) and non-LIS beneficiaries in Medicare Part D (Brown and Jeon 2022). Our work is distinct from theirs in that we are able to use random variation in the value of making an active choice.

cost-sharing levels and non-price rationing mechanisms (such as authorization restrictions) that they institute for covered drugs. In 2016, the average beneficiary had access to 26 stand-alone Part D plans. Beneficiaries are given access to official decision support tools ('Plan Finder'), that solicit information on the drugs they currently take and suggest the plans that best fit their needs.

While the Part D program is subsidized for all beneficiaries, those with financial need are granted additional subsidies through the low-income subsidy program (LIS), which provides premium and cost-sharing subsidies. Around 30% of Medicare beneficiaries participate in the LIS program. 'Dual-eligibles,' who simultaneously qualify for both Medicare and their state's Medicaid program, are automatically enrolled in the LIS program when they qualify for Medicare, as are beneficiaries in the Medicare Savings Program. Others who meet income and asset eligibility criteria can enroll by applying directly.

The dual-eligibles on which our study focuses receive generous subsidies.[5] This includes a subsidized reduction in their plan premium payments up to the 'benchmark' amount, which is set at the average of plans' premium bids in that region for that year. This subsidy covers the full premium amount for a subset of plans (known as 'benchmark plans'), making them free to dual-eligible beneficiaries. Dual-eligibles do not get to keep the difference if the subsidy exceeds the premium of their plan, and they must pay the remaining portion of the premium if they enroll in a non-benchmark plan. Beneficiaries have access to between two and sixteen benchmark plans, with 92% of beneficiaries having at least 5 benchmark plans to choose from (see Appendix Figure A11 for a histogram of available plans). Moreover, all cost-sharing features for covered drugs are effectively waived as a result of additional subsidies, which apply to both benchmark and non-benchmark plans. Instead, dual-eligibles face their own custom copayment schedule: In 2020, they were charged a copayment of $1.30 for covered generic drugs and $3.90 for covered branded drugs. This policy makes plans effectively uniform in their financial characteristics for dual-eligibles.

For dual-eligibles, plans thus differ primarily in the set of drugs covered by their formularies (the set of drugs that they offer coverage for).[6] Formulary exclusion of a drug means beneficiaries receive no insurance coverage for that drug. Because regulation requires plans to cover at least two drugs in each therapeutic class, beneficiaries will generally have another covered drug that they can switch to. However, these alternatives may not be perfect substitutes, making cross-plan differences in drug coverage meaningful for beneficiaries.

1.1.1 LIS Part D Default Rules

The LIS program has unusual default rules, designed to help beneficiaries maximize their subsidies. When an individual who already qualifies for her state's Medicaid program turns 65 and qualifies for Medicare, she is automatically enrolled in the LIS program. A few months before she enrolls in Medicare, she is assigned to a beneficiary-specific default plan that is chosen at random from among the set of stand-alone benchmark plans in her service region.[7] LIS beneficiaries are informed of their assigned plan and can opt out before their initial Part D enrollment is effectuated by actively selecting from among any of the plans available in

[5] Other LIS beneficiaries receive partial subsidization, which is less generous. We do not study that population in our analysis.

[6] As an example, of the nine benchmark plans in New York in 2009, six plans covered the drug Lipitor (atorvastatin) on their formulary while three did not. If an LIS beneficiary wanted to fill a prescription for Lipitor but was enrolled in one of the three plans that did not cover it, they could either pay for it fully out-of-pocket, substitute to another drug, or switch plans entirely.

[7] Randomization is done in a stratified random manner (with initial random assignment to insurers, then a second wave of randomization to plans within the assigned insurer) to prevent insurers from gaming the mechanism by introducing duplicate plans.

their service region.[8] Unlike unsubsidized beneficiaries, LIS beneficiaries can switch plans at the start of *any month*, not just during the yearly open enrollment period.

After initial enrollment, continuing LIS beneficiaries face a different set of default rules. In most markets for insurance products, including the unsubsidized component of Part D, the default is typically for the enrollee to remain in the plan they were enrolled in during the prior year. This is generally true in the LIS program as well, with one major exception: When plans lose their 'benchmark' status and are thus no longer free to LIS beneficiaries, the default for beneficiaries who had previously automatically enrolled in that plan by default switches from remaining in the prior plan to re-assignment to a randomly-selected benchmark plan. In October of year $t-1$, these beneficiaries receive notice that their default plan for year t is *not* their current (year $t-1$) plan but instead a plan that has been randomly chosen for them and that they will be automatically switched on January 1 if they do not opt out before then. This change in the default does *not* apply to beneficiaries who had actively chosen the plan that lost benchmark status. The motivation for this rule is to avoid automatically enrolling a beneficiary into a plan for which they would have to pay a premium for without their explicit approval.

Finally, beneficiaries enrolled in an plan in year $t-1$ that is no longer in the market in year t are also assigned to a random benchmark plan as a default. These defaults apply to both those who had previously been automatically enrolled in the exiting plan as well as those who had actively chosen it. This is distinct from the unsubsidized population, whose default upon plan exit is disenrollment from the Part D program.[9]

1.2 Data

We make use of several administrative datasets from the Centers for Medicare and Medicaid Services (CMS). These data contain information on beneficiary program enrollment status, medical utilization, and prescription drug utilization across the Medicare and Medicaid programs, linked longitudinally at a beneficiary level. The Medicare data is nationwide in scope and extends from 2007 to 2015. The Medicaid data covers the states of New York and Texas from 2004 to 2010.

Beneficiary Demographics, Enrollment, and Choice Status. We obtain information on beneficiary demographic characteristics and plan as well as program enrollment from the Medicare Beneficiary Summary File and the Medicaid Personal Summary File. These files provide demographic information such as age, gender, and geographic location, including the Part D plan region in which Medicare beneficiaries resided that year. We are able to track enrollment status in all Medicare programs (including the specific Part D plan of enrollment), Medicaid, and the LIS program at a person-month level.

We combine this data with a newly released plan election type file. This file covers all Part D enrollment spells from 2007-2015, and for each spell tracks whether enrollment was initiated through active choice or default auto-assignment. Importantly, in addition to listing the plan a beneficiary was enrolled in during

[8] See sample letters in Appendix Figures A17 and A18.

[9] The rules we have described in this section apply to most circumstances, with two exceptions. First, in cases where a beneficiary is being re-assigned, if their incumbent plan's insurer offers another benchmark plan in their service region, they will be routed to that plan rather than randomly assigned. Second, the state of Maine implements a 'beneficiary-centered assignment' mechanism where they use data on prior drug consumption to assign beneficiaries to plans that cover their drugs; however, our data does not record this assignment. We exclude both groups from our analysis.

each month, the file also includes the default plan that was assigned to the beneficiary, even if the beneficiary actively opted out of that default.

Plan Characteristics Data. We obtain information on plan characteristics from publicly available CMS datasets, which cover all Part D plans offered during our sample period. These data track the set of plans offered in each Part D plan region. For each plan in each year it was offered, we are able to observe the monthly premium that the plan charged (both the Medicare-paid portion and the beneficiary-paid portion), the plan's benchmark status, and the entirety of the plan's formulary.

Outpatient Prescription Drug Data. We track outpatient prescription drug usage for a random 20% sample of Part D enrollees using claims-level information from both the Medicare and Medicaid programs, based off the Part D Event files and MAX RX files, respectively. Each claim represents an event where a beneficiary filled a single prescription of a given drug. For each claim, we observe the specific drug prescribed and filled (at the NDC code level), the quantity/days supply for the fill, as well as the date when the prescription was filled, and the cost charged to the beneficiary and to the Medicare program.

Analytic Sample. Throughout the paper, we use a variety of subsamples of LIS beneficiaries for different analyses. For all of these subsamples, we employ a set of common restrictions. We restrict only to dual-eligibles (those simultaneously enrolled in Medicaid and Medicare, and thus receiving full LIS benefits) over 65 enrolled in Medicare Parts A, B, and D, and not enrolled in Medicare Advantage. We generally sample at the beneficiary-year level and require these restrictions to be true for every month in a year in which we include a beneficiary in our sample. We describe summary statistics for all beneficiaries captured in this analytic sample in the first column of Table 1.

2 Do New LIS Beneficiaries Follow Defaults?

We first provide descriptive evidence of default-following among LIS beneficiaries. These descriptives set the stage for our later analyses by showing just how rare active choice is in this setting.

We start by focusing on dual-eligible beneficiaries who have *newly* qualified for Medicare coverage by turning 65. As described in Section 1.1.1, this population is assigned a default benchmark plan three months before they qualify for Medicare coverage, and can opt out before they qualify, or switch plans at any point after they are automatically enrolled. We report summary statistics for our sample, restricted to those who turned 65 between 2007 and 2015, in the second column of Table 1. Given the relative youth and health of these individuals compared to our broader sample, these are individuals who should have greater cognitive ability than the average LIS beneficiary.

Panel (a) of Figure 1 presents the proportion of new LIS beneficiaries who actively opt out of their default, either through actively choosing a plan before automatic enrollment takes effect (in month zero), or by *actively* switching out of their plan after auto-enrollment (months one and on). We plot the cumulative share of beneficiaries who have opted out as a function of the number of months since their initial enrollment in Medicare. We restrict only to those for whom we observe five years of enrollment data following their

initial qualification for Medicare, i.e., those who aged into Medicare between the years of 2007 and 2010. We define active choosers here as those making independent, explicit choices of plans. We exclude re-*assignments* to new plans through the default reassignment mechanism so that this is a true measure of *cumulative active choice* rather than just a measure of persistence in the initial assigned plan. We average across all cohorts who enter Medicare during our sample window.

Initial opt-out is rare: Only 16% of new LIS beneficiaries make an active plan choice between being notified and their date of enrollment in Part D. The other 84% of beneficiaries enroll in their assigned default plan. This clearly shows that defaults matter in this setting. While these initial default rates are strikingly high, they could potentially be explained by the fact that beneficiaries can switch plans each month, causing them to not feel pressure to make a plan choice immediately, especially if they have limited drug needs. However, even after five years of enrollment, only 45% of all beneficiaries have *ever* made an active opt-out decision. This persistence is unexpected given that the initial default option was a *randomly-selected* plan which may not suit a beneficiary's needs.

To examine whether these low rates of active choice persist for various subgroups of beneficiaries, such as those with high drug needs or in particularly poor health, we construct a secondary sample made up of a subset of LIS beneficiaries for whom we observe Medicaid claims prior to enrollment in Medicare at age 65: those living in New York and Texas who aged into the Medicare program during the years 2007-2010. Restricting to this subsample allows us to construct *ex ante* measures of beneficiary characteristics using data from periods prior to Part D enrollment, eliminating the possibility that these measures capture plan effects on health and drug use. We report summary statistics for this sample in the third column of Table 1. In Appendix Figure A1, we replicate Figure 1 for this subsample. While these beneficiaries are less likely to make an active opt-out decision (with only 8% making an active opt-out decision before initial enrollment), they are otherwise qualitatively similar on other observable characteristics, except that they are slightly sicker.

Appendix Figures A2 and A3 stratify beneficiaries by health status and prior drug use, respectively. We find that sicker beneficiaries (in the sense of having more chronic conditions at age 64, as measured by the Elixhauser Comorbidity Index), and those taking more drugs at age 64, are somewhat more likely to make an active choice, although 1) even among the sickest, no more than 50% make an active choice within five years; and 2) the divergence only occurs within the first year of enrollment, and the groups trend in parallel for the four years following.

Beneficiary illness, however, is not a perfect measure of the incentive to make an active choice. A sick beneficiary whose drugs are covered by all plans has no incentive to make an active choice; moreover, sick beneficiaries may face higher opportunity costs of exerting effort if they are dealing with serious illness. To address this, in Figure 1b, we stratify beneficiaries in this sample by a measure of the potential value of making an active choice. We measure this in terms of the plan's 'fit,' the share of drugs it covers that the beneficiary was taking at age 64; specifically, its ranking (in quintiles) relative to other available benchmark plans. A plan with poor fit will force beneficiaries to pay the full price out-of-pocket to take the drugs they were previously taking. Thus, the value of making an active choice should be higher for those assigned default plans with relatively poor fit. Strikingly, the relationship between the relative fit of a beneficiary's randomly-assigned default plan and her propensity to make an active choice is *not even monotonic*. We

discuss this measure of plan 'fit' further in Appendix B.[10]

These stylized facts provide clear evidence that default assignments matter for beneficiary plan enrollment in this setting. None of the subgroups of beneficiaries most likely to benefit from choice as measured by health status, use of drugs, or the relative formulary fit of their assigned default plan have initial rates of active choice above 10%. Further, all of these groups have rates of active choice below 50% *even after 5 full years in the program*. This occurs despite the fact that the initially-assigned plan was selected randomly. Our results also rule out the standard neoclassical explanation for persistent enrollment in the same plan over time; i.e., that beneficiaries make an initial active choice and stick with it over time because their preferences do not change (Keane 1997). Since the initial enrollment in our setting was random, rather than chosen, this hypothesis cannot explain our results.[11]

3 Do Continuing LIS Beneficiaries Follow Defaults?

Our results in Section 2 show that default assignment matters for initial enrollment. However, we cannot necessarily conclude that it explains the fact that these default assignments persist across time. This persistence may come either from continuing to follow the default (to remain in the assigned plan), or because beneficiaries face "switching costs" (Handel 2013, Polyakova 2016). The latter explanation says that beneficiaries face costs (administrative, cognitive, or otherwise) to switching plans, and that in each period beneficiaries are *actively* choosing to remain in their prior plan in order to avoid incurring those switching costs, rather than passively following the default. This is not a distinction without a difference: If the patterns we see are driven by beneficiaries actively remaining in their plans to avoid switching costs, then we should expect changes to the default to have no effect on enrollment. In this section, we provide a clean test for showing that default effects explain enrollment persistence.

3.1 Switching Costs vs. Default Effects

Distinguishing between default effects and switching cost explanations is impossible when the default option is for the beneficiary to re-enroll in her incumbent plan (Handel 2013). Both sets of mechanisms will lead to the same observable outcome: Beneficiaries will persistently remain in their plans, even when they could benefit from switching. Instead, in order to adjudicate between the two, we need to show that an exogenous change in default rules affects persistence. If it does, we can reject the model of switching costs in favor of a default effect explanation.

[10] We show in Appendix Figure A19 that there is significant variation in fit, ranging from 0% to 100% coverage, with substantial mass in between. In Appendix Figure A20 we show that, for a given person, the difference between the average and the best benchmark plan in terms of fit can be a difference of as large as 40% or more of their age 64 basket of drugs. In Appendix B we show that all of these results hold when using a fit measure that weights drugs by their average transacted price.

[11] In practice, some beneficiaries will be randomly assigned to their most-preferred plan, and we will miscategorize them as being passive when they actively chose to remain in their assigned plan. This cannot rationalize our results. The probability of being assigned to one's own personal most-preferred plan is $\frac{1}{N}$, where N is the number of plans. Even with $N = 2$, the smallest number of *benchmark* plans ever available in any region, this probability is $\frac{1}{2}$, less than the share of assigned beneficiaries who fail to opt out even after five years. In general, with a default opt-out rate of R, we can bound the default-following rate due to inattention from below by $1 - R - \frac{1}{N}$.

The default rules for continuing LIS beneficiaries provide exactly this type of exogenous change. For beneficiaries who were previously auto-assigned to their plan, the typical default is to remain in their incumbent plan. This default remains in force *unless the plan loses its benchmark status by setting a year t premium above the year t subsidy*. If a plan that was a benchmark plan in year $t-1$ sets its premium in year t above the year t subsidy, the default for its incumbent enrollees switches from remaining in the plan to being auto-enrolled in a *different*, randomly-selected year t benchmark plan. This setting provides us with a clear test of whether persistence is driven by defaults: If so, a change in the default rules will induce beneficiaries to switch plans through auto-assignment. In contrast, if persistence is driven by switching costs, beneficiaries whose default changes to a different randomly-chosen benchmark plan will actively choose to remain in their incumbent plan in order to avoid incurring the switching cost.

In order to deal with the possibility that beneficiaries enrolled in plans that lose benchmark status may differ from beneficiaries enrolled in plans that do not, we implement this test via a regression-discontinuity (RD) design. Specifically, we compare year-to-year plan switching rates for beneficiaries whose incumbent plans set premiums just below the subsidy, and therefore retained their benchmark status, versus beneficiaries whose incumbent plans set premiums just above the subsidy, losing their benchmark status.

Research Design. For this analysis, we create a subsample of all beneficiaries who were enrolled in a benchmark plan in December of year $t-1$ and had been enrolled in that plan through the auto-assignment mechanism, i.e., those who face a potential default rule change, and whose $t-1$ plans continued to exist in year t. We summarize this subsample in the fourth column of Table 1. This subsample is slightly younger than the general LIS population, and has slightly less medical utilization, but is otherwise similar.

Our research design is based on comparing the switching behavior of beneficiaries whose incumbent plan's year t bid was just under the year t subsidy level to those whose plans bid just above the subsidy level. This design is valid under the assumption that whether a plan bids just above or just below the subsidy level is orthogonal to the latent propensity of beneficiaries to make an active choice. One might worry that some strategic plans might bid just under the subsidy level to avoid shedding any default-following customers. However, the subsidy is set *after* plans have already set their premiums, as an average of plan premium bids within each Part D region. While plans would prefer to be just below the subsidy and therefore receive the maximum subsidy amount without losing any default-following customers, we assume that they cannot *perfectly* foresee what the subsidy will be, and their position relative to the benchmark cutoff thus represents random forecasting error. In practice, plans frequently lose benchmark status from one year to the next, with, for example, 44% of all 2008 benchmark plans losing that status in 2009.

To assess our assumption of quasi-randomness, in Figure 2, we plot a histogram of the incumbent plans' premiums (relative to their regional subsidy level) in the following year, in the spirit of the density test of McCrary (2008). Approximately 68% of beneficiaries are enrolled in a plan that set a premium below the cutoff.[12] 12% are enrolled in 'de minimis' plans which bid above the cutoff but were allowed to revise their

[12]The higher likelihood of being below the cutoff is a consequence of our requirement that our sample only contains plans that were benchmark plans in the prior year and thus all had premiums below the subsidy. A rational forecast of their future prices would be for the mean to remain under the subsidy amount with some variation. In Figure A4 we present a scatterplot relating premiums in $t-1$ to premiums in t for *all* plans that continued to exist in consecutive years. The conditional mean of the premium bid in t relative to the cutoff is approximately a linear function of the premium bid in $t-1$.

bids down to the subsidy amount after the subsidy determination.[13] Importantly, the distribution of plan premiums appears to be relatively smooth across the discontinuity.

Figure 2 does show that there is a greater density of plans just to the left of the subsidy cutoff. While in many regression discontinuity designs this can serve as a threat to identification, it is not particularly concerning in this setting. First, we measure the outcomes of *beneficiary* actions, but the running variable is entirely determined by *plans*, not the beneficiaries themselves. Second, bunching is likely to be driven by plans trying to stay under the threshold to retain passive beneficiaries. Our estimates may be biased if plan efforts to do so are correlated with whether incumbent beneficiaries in those plans are likely to follow the default. However, plan effort should be higher if their incumbent beneficiaries are *more* likely to be passive. Therefore, beneficiaries who end up treated would be *less* passive than average, implying our (already large) estimates would be smaller than the true average treatment effect.

We also perform a number of balance tests to show that beneficiary characteristics are comparable across the discontinuity. In Figure 3, we plot average drug spending in the prior year as a function of the incumbent plan premium bid relative to the subsidy and find that it is smooth around the discontinuity. We find similar balance for average age, female share, average non-drug medical spending in the prior year, and average Elixhauser Comorbidity index, which are plotted in Appendix Figure A7. Regression estimates for differences in these characteristics across the discontinuity are found in Appendix Table A1.

Regression Discontinuity Results. To implement our test, for each beneficiary in our sample we measure whether or not the beneficiary retained the same plan between December of year $t-1$ and January of year t. In Figure 4, we plot the average switching rate for whole-dollar bins of the difference between each incumbent plan's premium in year t and the regional subsidy in that year. All 'de minimis' plans are dropped. Our results are striking: the average probability of switching among beneficiaries on the left side of the cutoff, where the default is to remain in the incumbent plan, is less than 1%. In contrast, the average probability for beneficiaries on the right side of the cutoff, where the default is to be reassigned, is approximately 97%. When the default changes from re-enrollment in one's prior plan to re-assignment to a different plan, beneficiaries switch plans, rather than actively choosing to remain in their incumbent plan to avoid incurring a switching cost.

While these results clearly show that defaults affect enrollment, thereby ruling out the switching cost hypothesis, they do not necessarily show that beneficiaries are actually following the default. Some individuals might instead actively switch to a plan that is not their randomly-chosen default plan. Appendix Figure A8 replicates Figure 4 but breaks out the switching rates into active switches and passive switches, as observed in the data. Beneficiaries are, on average, 2.4 percentage points more likely to make an active choice to switch when their incumbent plan's premium in year t is on the right side of the year t cutoff. This increase in active choice is small compared to those who switch passively via the default auto-assignment

[13] This refers to a policy that CMS adopted in 2011. Plans whose bids were less than or equal to $2 above the subsidy were allowed to, ex post, revise their bid down to the subsidy to stop their enrollees from being re-assigned. In practice, nearly every plan exercised this option. To be conservative, we drop all plans that bid in this range when these rules were in effect. In Appendix Figures A5 and A6 we recreate this histogram from the pre- and post-de minimis time periods. The pre-de minimis time period still has a few de minimis plans, owing to the fact that CMS experimented with de minimis rules in a very small number of markets during 2007 and 2008. To be conservative, we drop plans that bid in the de minimis range when/where these policies are in effect.

mechanism.[14]

Next, we estimate the change in the probability of switching at the discontinuity via regression. We follow Lee and Card (2008) and estimate a global conditional expectation function to approximate the switching rate for beneficiaries whose plans' premiums were above the subsidy level in the counterfactual scenario where default rules do not change. To do so, we estimate the following linear probability model for an individual i in year t, previously enrolled in plan j in region r, restricting to beneficiaries enrolled in plans with benchmark status in year $t-1$ whose bids for year t fall within \$6 of the year t benchmark:

$$\begin{aligned}
\text{Pr(Switch Plans)}_{it} &= \beta \cdot 1\{Bid_{j(i)t} - Benchmark_{r(i)t} > 0\} \\
&+ \gamma^{-} \cdot (Bid_{j(i)t} - Benchmark_{r(i)t}) \cdot 1\{Bid_{j(i)t} - Benchmark_{r(i)t} \leq 0\} \\
&+ \gamma^{+} \cdot (Bid_{j(i)t} - Benchmark_{r(i)t}) \cdot 1\{Bid_{j(i)t} - Benchmark_{r(i)t} > 0\} \\
&+ \delta X_{it} + \epsilon_{it}
\end{aligned}$$

Our parameter of interest is β, which we interpret as the effect of the change in default rules. We present the coefficient estimates from our regression in Table 2.[15] In columns 2-4, we divide the effects into active switching, active switching to a benchmark plan, and passive default reassignment (which, by construction, never occurs for beneficiaries in plans below the cutoff). Our regression results echo those from our figures: Incumbent plan benchmark status loss results in a substantial share of beneficiaries switching plans (96%), primarily through default reassignment (93.6%), with only a trivially small portion (2.4%) of beneficiaries switching plans actively in response to the default change.

Alternative Explanations. One concern with interpreting β as purely the effect of a change in defaults is that remaining in a plan whose premium is above the benchmark requires the enrollee to pay a positive premium equal to the difference between the plan's year t bid and the year t benchmark. This difference is small, only amounting to a few dollars per month compared to \$0. However, as prior research has shown, changing prices from \$0 to \$1 may have large effects due to the salience of zero (Shampanier et al. 2007) or due to transaction costs making it difficult for beneficiaries to pay even small positive premiums (McIntyre et al. 2021, Drake et al. 2021). To explore this, we redo this analysis on a sample of beneficiaries in the same plans who had initially *actively* chosen those plans. For these beneficiaries, the default is *always* for the beneficiary to remain in their incumbent plan, whether or not it remains a benchmark plan, so any difference in plan switching across the discontinuity only reflects price sensitivity and not default effects. In Appendix Figure A9 we take our plot from Figure 4 and overlay the plan switching rates for these "prior chooser" beneficiaries. We do see an increase in the switching rate at the discontinuity for these prior choosers despite the lack of a change in the default: The switching rate increases discontinuously by about 20% at the cutoff. This does provide evidence that beneficiaries do exhibit sensitivity to positive premiums. However,

[14] In Figure A8, we also plot the share of beneficiaries who make an active choice and switch to a *non-benchmark* plan. This is a trivial proportion of switches (0.4pp).

[15] Lee and Card suggest clustering standard errors at the level of discrete running variable value. We follow the spirit of this inference approach by clustering standard errors at the incumbent-plan-by-year level.

this sensitivity accounts for at most 20-25% of the effect observed for the beneficiaries in our main sample, assuming price sensitivity is similar for these two groups.[16]

Another concern is that beneficiaries can switch plans at any time, so accidental assignment to the default mechanism may be reversed the following month when beneficiaries realize they have been switched. In this case, our approach would overestimate the effects of defaults on (long-run) plan switching by only looking at enrollment in January. We address this by performing the same analysis but instead comparing enrollment in December of year $t-1$ to enrollment in *December* of year t. We present the associated plot in Appendix Figure A10. Our estimates are nearly identical to those that use January enrollment, as only 0.5% of reassigned beneficiaries switch back to their previous plan during year t. In Appendix C we extend the analysis through 24 months after the implementation of the new default and show that these default effects persist throughout that entire 2 year period.[17]

Finally, one might argue that our results are based on beneficiaries who had previously been auto-assigned to a plan, and thus revealed themselves to be more passive than the average beneficiary; therefore our results might not generalize. We note that, at any given point in time, two-thirds of LIS beneficiaries are in a plan they were auto-assigned to, so this group represents the majority of the population. Additionally, in Section 7, we use a different natural experiment that includes prior active choosers and show an overwhelming majority of them also follow the default.

Interpretation. Our interpretation of the sum of our results in this section and in Section 2 is that both initial enrollment, as well as the year-to-year persistence of beneficiary enrollment in a given plan, are both driven by beneficiaries passively following default rules. In turn, we can reject the hypothesis that switching costs drive these patterns. To clarify, our results do not suggest that real ramifications of switching do not exist; they merely reject the hypothesis that beneficiary choices respond to those costs when they arise. Our results show that most beneficiaries are not even willing to pay $3 per month to remain in their incumbent plan. This occurs even when potential search frictions can be easily overcome: While searching for a new plan may be difficult, beneficiaries can pay this small premium to simply remain in their incumbent plan and know that their existing set of drugs will continue to be covered.

4 Should Beneficiaries Care About Default Rules?

In Sections 2 and 3, we show that default rules matter in determining what plans beneficiaries enroll in. However, an existing literature on retirement savings has emphasized that changes in enrollment need not induce changes in actual outcomes (Choi et al. 2004, Choukmane 2021, Beshears et al. 2021a, 2022). In this section, we show that default rules have real consequences for beneficiary drug consumption.

[16] As an aside, we note that the effect of premiums moving from $0 to $1 on switching exhibited in this robustness check is extraordinarily large; much larger than either the effect of moving from $1 to $2, and also larger than the effect of changes in formularies that induce hundreds of dollars in consumption losses that we analyze in Section 6.

[17] The barrier to doing this two-year follow-up in our main specification is that many beneficiaries in both our treatment and control groups are in plans that lose benchmark status the following year, and thus may face a second reassignment within the analysis window. Interpreting treatment effects in such a context is challenging. In Appendix C we solve this by restricting only to those beneficiaries whose assigned plans do not lose benchmark status in the following year. However, this restriction reduces our sample size substantially.

To estimate the effect of default rules on drug consumption, we again leverage the natural experiment from Section 3. Some beneficiaries quasi-randomly faced a default of being reassigned, due to their incumbent plans setting premium bids just above the benchmark, while other control beneficiaries faced a default of remaining in their plans due to their incumbent plans setting premium bids just below the benchmark. We compare these two groups in a difference-in-differences approach, contrasting the differences in the two groups' drug utilization in earlier years (when both faced a default of remaining in their plans) to later years (when the default rules faced by each group diverged).

We take a stacked difference-in-differences approach (Cengiz et al. 2019). For each market (Part D geographic region) and year, we identify all beneficiaries who were enrolled in a benchmark plan in year $t-1$ that submitted a year t premium bid within \$6 of the year t subsidy. Within each of these market-year pairs, we have a valid difference-in-differences research design 'experiment' indexed by d. We truncate the event time windows of each experiment to 8 quarters before the potential default change and 4 quarters after. For each, the resulting experiment-level dataset only includes beneficiaries who were continuously enrolled in their incumbent plans for the preceding 8 quarters.[18] Our final sample is composed of 464,557 person-by-experiment observations, over 12 quarters.

We then stack the resulting experiment-level datasets and estimate a pooled regression:

$$y_{itd} = \beta(BenchmarkLoss_{id} \times Post_{td}) + \gamma_{id} + \eta_{td} + \epsilon_{itd} \quad (1)$$

where y_{itd} is the outcome of interest for person i in quarter t in experiment d, $BenchmarkLoss_{id}$ is a dummy variable equal to one if person i is in the treatment group in experiment d (i.e., that their incumbent plan submitted a bid which would cause it to lose benchmark status), and $Post_{td}$ is a dummy variable equal to one if time t is after the potential change in default occurs in experiment d. γ_{id} is an individual-by-experiment fixed effect, recognizing that an individual may appear in multiple experiments. η_{td} is a full set of event-time-by-experiment fixed effects.

This approach is equivalent to estimating a series of experiment-specific difference-in-differences regressions, and constructing a sample-weighted average of the resulting estimates. We enforce that the estimator can only incorporate comparisons *within-experiment* between the treated group and an explicitly-designated control group. One quirk in our approach is that beneficiary-year observations frequently appear multiple times in the pooled dataset. We thus cluster standard errors at the level of the beneficiary, rather than the beneficiary-by-experiment level.

This research design requires that drug consumption by control beneficiaries serve as a good counterfactual for the consumption of treated beneficiaries if they had not faced reassignment. One concern is that these plans evolve over time both in their premiums and in their formulary design, and so treated beneficiaries' incumbent plans may have changed in ways that cause them to diverge from control beneficiaries' incumbent plans. In the last column of Appendix Table A1, we rule out this possibility by using our regression discontinuity design from Section 3.1. We measure what the 'fit' would be if the beneficiary stayed in their incumbent plan; that is, what share of the drugs consumed by the beneficiary in the prior year are covered

[18] This means that their incumbent plan must have retained benchmark status for the prior two years. This ensures that beneficiaries did not recently face a different reassignment event. As Sun and Abraham (2021) point out, if treatment effects evolve over time, including recently-treated units as controls can generate bias.

in their incumbent plan's formulary in the current year. We find that plans that bid above the benchmark threshold have slightly *worse* fit, although differences are small.

Results. Our primary outcome measure, y_{itd}, is the log of the beneficiary's total allowed drug spending in a quarter. This spending includes both what the beneficiary paid out of pocket (typically very little) as well as what Medicare, Medicaid, and the insurer paid on their behalf. Panel (a) of Figure 5 shows the event study version of Equation 1 (allowing a separate β to be estimated for each quarter) for overall drug spending, with T as the quarter of the change in default rules. We see a sudden drop in drug spending for beneficiaries whose plan loses benchmark status in the year after this benchmark status loss occurs (after T). Encouragingly, we do not see any differences between these groups in the years *before* the benchmark status loss. In the first column of Table 3, Panel A, we present our estimate of β from Equation 1, where we pool across all quarters in the pre- and post-periods. The loss in benchmark status and the subsequent change in the default results in a 6.4% drop in drug spending for affected beneficiaries, approximately $213 off a base of $3,329, a nontrivial loss in valuable consumption. In Appendix C we show that this reduction persists at least 24 months after the implementation of the new default, suggesting that the mismatch is permanent in nature rather than a transitory reaction to new coverage barriers.

In Panel A of Table 3 we investigate the extent to which these consumption results are sensitive to how we measure consumption. In the second column, we take our initial dataset and reprice all the claims to reflect the average drug-level (NDC) price in that year across all plans, so that we only capture differences in actual consumption and not cross-plan differences in price. While the effects are slightly smaller (5% reduction vs. 6.4%), they remain similar. However, even with this normalization in place, observed reductions in drug *spending* may merely reflect substitutions from higher- to lower-priced drugs that treat the same ailment. By counting this as a loss in consumption, we implicitly assume that this change represents a reduction in value to the beneficiary. In the third column, we reprice each drug to have the average price of any drug in its therapeutic class, across plans in a given year. This estimate reflects consumption losses under the extreme assumption that all drugs within a therapeutic class are perfect substitutes. In practice, this is generally not true,[19] and so the estimate serves as a sharp lower bound for the importance of the consumption effects. Under this normalization, we find consumption reductions of 2.1%.[20]

In Panel A of Table 4 we focus on consumption effects for specific types of drugs.[21] In the first column, we focus on high-value drugs, finding a reduction identical to our main estimate, indicating that the results are not driven entirely by decreases in low-value consumption. We also see larger effects for chronic drugs than acute drugs, which may be expected as plan transitions are more likely to affect chronic medications. Finally, in the third column of Table A2 we investigate effects on non-drug medical consumption, a potential

[19] For example, one therapeutic class is antipsychotics; our definition of therapeutic class bundles together cheap but less-effective first-generation antipsychotics with expensive but more-effective second-generation antipsychotics.

[20] In Table A2 we also measure the effects of the default change on the total number of prescription fills and days supply. These effects are relatively small; however, interpreting them requires the even more extreme assumption that a unit of *any* drug has equivalent value. Given that managed care restrictions used by plans are primarily imposed on new drugs (Brot-Goldberg et al. 2022), this assumption seems unrealistic.

[21] We characterize drugs as high-value based on whether they belong to therapeutic classes targeting key chronic conditions: diabetes, depression, hypertension, and asthma. We define chronic drugs as drugs for which the median number of annual, per person fills is greater than two, among the subset of people with at least one annual fill (Einav et al. 2015).

proxy for health effects of changes in drug consumption (Chandra et al. 2010). Ultimately, we find that reassignment increases non-drug spending, but the effects are small and noisily estimated, making them difficult to interpret.

This section's results show that differences in potential default rules have substantial consequences for beneficiaries' access to drugs. The 'wrong' default could lead to substantial changes in what drugs a beneficiary takes, and, in the long run, potentially on their health.

5 Theoretical Framework

In the prior sections, we provided evidence that default rules have powerful effects on enrollment and utilization among LIS beneficiaries, not just when those default rules keep them enrolled in the same plan, but also when those rules switch their enrollment across plans. We now know that many 'choices' in this setting are passive, not active. The question is, how should a policymaker evaluate the potential welfare losses of this passivity, and what (if anything) should they do about it?

To answer this question, we need a model of beneficiary passivity. While understanding the exact model of behavior would be ideal, the space of models that generate default effects is so large that finding a single 'true' model is practically impossible. However, progress can still be made by considering large classes of models with similar features. The two primary classes of models of passivity are, in the terminology of Handel and Schwartzstein (2018), 'frictional' models and 'mental gap' models. Most of the literature on defaults considers frictional models, where agents make an active choice if the perceived expected value to the agent of making an active choice exceeds the 'decision cost', or the cost of the effort required to overcome the behavioral friction and make the active choice. Such models include rational attention formation (Gabaix 2014, Matějka and McKay 2015), rational search (Honka 2014), as well as models with flat utility penalties to making an active choice, the latter being especially common in the health insurance (Handel 2013, Polyakova 2016) and retirement savings (Choi et al. 2003, Carroll et al. 2009, DellaVigna 2018) literatures. Mental gap models, in which active choice is driven by contextual salience or norms rather than material incentives, are less common in this literature.

In this section, we show that these two classes of models differ greatly in their implications for the welfare consequences of passivity and optimal policy responses. We then attempt to derive testable implications between these two general classes of frictional and mental gap models to allow us to identify key empirical parameters that will help us adjudicate between different classes of models. In the remainder of the paper, we then attempt to estimate these key parameters. In this section, we will cover the minimum necessary details of the two classes of models to motivate our later empirical sections. Interested readers can read Appendix D or Handel and Schwartzstein (2018) for more detail.

5.1 Framework

We start by laying out a framework for discrete choice with costly consideration. Agents are given a set of options to choose from, each with an associated value. An agent has two paths towards enrollment: They may either actively choose a plan of enrollment, or passively accept the default option. If they make an

active choice, they receive a payoff v^* based on the private value of their choice. If they fail to make an active choice, they are assigned the default, and receive a payoff v^d. Making an active choice is costly, with decision cost c. c represents the various frictions associated with making an active choice, such as psychic costs of attention, effort costs of searching for information about the available options, and/or opportunity costs of time spent doing so.[22] The net value to the agent of making an active choice (relative to passively following the default) is $v^* - c - v^d$.

In deciding whether or not to make an active choice, the agent follows a decision rule: They make an active choice if and only if $A(v^*, v^d, c, ...) \geq 0$, where $A(\cdot)$ is an arbitrary function of v^*, v^d, c, as well as other factors, both deterministic and stochastic. We conceptualize various models of active decision-making by putting parametric structure on $A(\cdot)$. In 'frictional' models of default-following behavior, $A(\cdot)$ is often the net value of making an active choice, $v^* - c - v^d$, as it is in Handel (2013) and Polyakova (2016), as well as models of rational search or attention (Honka 2014, Matějka and McKay 2015). Alternative 'frictional' models may replace the true net value of active choice with a perceived net value where agents weight the decision attributes incorrectly, such as the 'beta-delta' models of present bias used in the retirement savings literature (Choi et al. 2003, Carroll et al. 2009, DellaVigna 2018) or models where agent attention responds only to the attributes of the default, like those of Ho et al. (2017) and Abaluck and Adams-Prassl (2021). In contrast, in 'mental gap' models, $A(\cdot)$ is divorced from these material factors, either because active choice is driven by factors like memory, framing, and random shocks to attention, or because attentive or search effort depends on prior beliefs which are independent of the ground truth and are instead driven by norms or previous experience. In Appendix D we discuss parameterizations of $A(\cdot)$ in the literature in more detail.

5.2 Welfare and Optimal Defaults

Distinguishing between models is important to the extent that it informs welfare and policy implications. To assess this distinction, we consider a utilitarian social welfare function that aggregates realized agent values for their choices.[23] Welfare under our framework is simple: If an agent makes an active choice, they receive a final payoff $v^* - c$, whereas if they follow the default, they receive payoff v^d. Therefore, expected welfare is given by

$$\begin{aligned} W &= \Pr\{A(v^*, v^d, c, ...) \geq 0\} \cdot E[v^* - c | A(v^*, v^d, c, ...) \geq 0] \\ &+ \Pr\{A(v^*, v^d, c, ...) < 0\} \cdot E[v^d | A(v^*, v^d, c, ...) < 0] \end{aligned}$$

As mentioned above, the key distinction between frictional and mental gap models of default effects is the extent to which active choice is a function of material incentives. Under our framework, this distinction is captured by the derivative of $A(\cdot)$ with respect to v^d, the payoff value from passively accepting the default.

[22] Handel (2013) and Goldin and Reck (2022) discuss welfare consequences when (their equivalent of) c is or is not welfare-relevant for a policymaker. We will assume it is always welfare-relevant.

[23] Readers should turn to Bernheim and Rangel (2009) or Bernheim and Taubinsky (2018) for a detailed treatment of how one might approach quantifying welfare when agents have potentially inconsistent preferences and the concerns of various strategies for doing so. We assume that there is a consistent welfare measure that has already been chosen.

To show that this distinction is important for the welfare economics of default effects, it is instructive to differentiate W with respect to v^d:

$$\frac{\partial W}{\partial v^d} = \underbrace{1 - a(v^*, v^d, c, ...)}_{\substack{\text{Benefit to} \\ \text{inframarginal} \\ \text{passive agents}}} + \underbrace{\frac{\partial a(v^*, v^d, c, ...)}{\partial v^d}}_{\substack{\text{Marginal agents} \\ \text{induced to not make} \\ \text{an active choice}}} \underbrace{E[v^* - c - v^d | A(v^*, v^d, c, ...) = 0]}_{\text{Value of choice for marginal agents}} \tag{2}$$

with $a(v^*, v^d, c, ...) = \Pr\{A(v^*, v^d, c, ...) \geq 0\}$.

As the value of the default, v^d, increases, there are two opposing effects on W: First, the inframarginally passive agents, who receive the default option, benefit by the exact amount of the increase; the share of these agents is $1 - a$. Second, there are marginal agents, who are indifferent between making an active choice and remaining passive. The increase in the net benefit of remaining passive may induce some agents to passively accept the default rather than make an active choice, with the share of these agents given by $\frac{\partial a}{\partial v^d}$, the attention elasticity, and their welfare changes by the net value of making an active choice. The value of $\frac{\partial a}{\partial v^d}$ reflects the key frictional versus mental gap distinction from Section 5.1: A purely frictional model would predict that $\frac{\partial a}{\partial v^d} < 0$, as agents respond to weakening material incentives to make an active choice; a model purely focused on mental gaps would predict that $\frac{\partial a}{\partial v^d} = 0$.

The value of $\frac{\partial a}{\partial v^d}$ (and thus the class of models of passivity) is important for at least two issues in the welfare economics of defaults. First, it determines the worst-case bounds for welfare as the value of the default degrades. In a frictional model, welfare is generally strictly bounded from below; for example, in a rational inattention model like that of Handel (2013), the worst possible outcome for an agent is $v^* - c$, as an agent in this model will not tolerate a default that is worse than their outside option of paying the decision cost c and making an active choice. These bounds grow larger, but still remain, even in boundedly-rational models like beta-delta models of present bias. Alternatively, an agent whose attention is random, or merely triggered by non-material factors such as salience or norms, may follow a default into oblivion, since worse defaults do not necessarily trigger active choice. While others have pointed out that agents in models with mental gaps—such as models of present bias with naivete (O'Donoghue and Rabin 2001) or non-Bayesian learning (Hanna et al. 2015)—have unbounded potential welfare losses, we note that the boundedness of welfare losses is a general feature of frictional models, rather than a particular feature of specific models. In contrast, models with mental gaps may or may not have this property.

Second, the value of $\frac{\partial a}{\partial v^d}$ is important for considering optimal default rules. We can think about different default rules as implementing different values of v^d. In this view, our Equation 2 can be viewed as a first-order condition for optimal default-setting, akin to similar formulae for optimal taxation. A small literature has concluded that under certain market conditions, the optimal default is one that minimizes v^d, to maximize active choice (Carroll et al. 2009). The logic is that, if $E[v^* - c - v^d | A(v^*, v^d, c, ...) = 0] > 0$, agents on the margin of making an active choice will be making a mistake by not doing so, and therefore a policymaker should implement a default so shockingly bad for the agent that all agents make an active choice. The focus of this literature has been normative: Bernheim et al. (2015) and Goldin and Reck (2022) show that different normative assumptions about whether passivity is a mistake or a response to real decision costs can generate wildly different conclusions for optimal policy, ranging from 'shocking' defaults

that minimize v^d to 'smart' defaults that try to maximize v^d, as suggested by Thaler and Sunstein (2008) and Handel and Kolstad (2015a). However, this prior work has taken frictional models of default effects as a given. A shocking default only works if agents respond to reducing the default quality by making an active choice. If, instead, $\frac{\partial a}{\partial v^d} = 0$, as it would under 'mental gap' models of default effects, the second term in the above equation drops out, and the optimal default is always one that seeks to maximize v^d. We explore the welfare economics of specifications of $A(\cdot)$ in more detail in Appendix D.

5.3 Testable Implications and the Path Forward

Given that the distinction between positive models of default effects matter for welfare and policy, the question is how to empirically test between these positive models.[24] As shown above, the key distinction is between frictional and mental gap models of passivity, and those models are differentiated primarily by whether changes in the value of following the default affect the propensity to make an active choice, that is, the value of $\frac{\partial a}{\partial v^d}$. Thus, the primary parameter useful for testing between these two classes of models is $\frac{\partial a}{\partial v^d}$, with $\frac{\partial a}{\partial v^d} < 0$ providing evidence in favor of the frictional class of models, and $\frac{\partial a}{\partial v^d} = 0$ providing evidence against. To estimate $\frac{\partial a}{\partial v^d}$, we require sufficient variation in v^d, i.e., different default options must implement sufficiently different realized values for agents. Thus, in Section 6, we provide evidence that the various plans that LIS beneficiaries can be assigned as defaults vary significantly in the value they provide to beneficiaries, thus providing sufficient variation in v^d. Then, we leverage that variation to estimate $\frac{\partial a}{\partial v^d}$.

Finding $\frac{\partial a}{\partial v^d} \approx 0$ (as we do) is useful evidence, but insufficient for completely rejecting a frictional model of default effects. In Appendix D, we show that heterogeneity in decision costs c can cause there to be some agents whose cognitive costs are so high that they do not make an active choice under any observable default; similarly, there also may be some agents whose costs are so low they make an active choice no matter what the default is. If c is distributed with cumulative density function F, then if we observe those two defaults, v_1^d and v_0^d, the share of beneficiaries induced to make an active choice from the change in defaults will be $\Delta = F(v^* - v_1^d) - F(v^* - v_0^d)$. While this implies that a single estimate $\frac{\partial a}{\partial v^d}$ cannot allow us to reject all frictional models, it also implies that our estimate of $\frac{\partial a}{\partial v^d}$ can allow us to reject *some* frictional models. Specifically, given the two defaults v_1^d and v_0^d, we can reject all frictional models where c has probability density of Δ or more between $v^* - v_0^d$ and $v^* - v_1^d$. If the variation in v^d is small relative to the variation in c, we would only be able to reject a small subset of frictional models. But given sufficient variation in v^d, this exercise can (and in practice does) allow us to reject most of the models in the frictional model space, potentially only leaving frictional models with implausible distributions of decision costs. If that is the case, this will provide strong evidence against frictional models.

However, it could still be the case that frictional models are a correct description of the world, but that the interval $[v^* - v_0^d, v^* - v_1^d]$, i.e., the range of possible consequences for not making an active choice, is short and therefore does not include most of the density in c. To the extent that c is constant within agents, a frictional model implies that agents should either be 'always choosers' or 'never choosers.' This

[24] Given that we previously found overwhelming passivity despite potential losses in drug consumption, one might ask why that result alone does not reject frictional models. One possibility is that, given that health care use is highly right-skewed, the small number of 'choosers' may also be the beneficiaries who have by far the most to lose, with other beneficiaries facing relatively small stakes.

should occur due to the fact that most beneficiaries either face no frictions, and thus always make an active choice, or frictions that are so extreme that no possible default they face should ever encourage them to exert effort to make an active choice. Within-individual variation in attention should therefore mostly reflect the contribution of non-material factors. We explicitly decompose the variation in attention in Section 7.

6 Do Worse Defaults Drive Beneficiaries to Make Active Choices?

In this section, we estimate how the beneficiary's propensity to make an active choice responds to the value of the default option. Estimating this response requires three steps. First, we must identify exogenous variation in default options. Next, we must show that this variation in default options induces variation in the value of the default, v^d. Finally, we must estimate how much the active choice propensity varies in response to v^d.

Measuring beneficiary value for different options is challenging: While econometricians normally use revealed preference approaches, those will fail when beneficiaries are not making active choices and thus not revealing their preferences. Instead, we ask whether beneficiaries face *different consequences* of enrolling in different default options, in terms of the drug consumption they achieve in that option. We think of this approach as an ex post measure in the spirit of other recent research measuring welfare effects of health insurance policies based on their effects on consumption (Finkelstein et al. 2019).

We have already shown in Section 4 that different default options induce different levels of drug consumption. However, the comparison in that section cannot be used for these purposes, since we are unable to measure active choice among the 'control' group whose default is to stay in their incumbent plan. Instead, we use the fact that, among 'treated' beneficiaries whose default changed to being re-assigned, their assigned plan was chosen at random. For some beneficiaries, this assigned plan covered all of their previously-taken drugs, while other beneficiaries faced large coverage gaps and disruption.

6.1 Differences in Valuation of Randomly-Selected Default Plans

We measure the potential valuation of a default plan in terms of its 'fit' for the beneficiary. We measure fit as the share of the prescriptions taken by the beneficiary prior to the change in the default that are covered by the plan's formulary. For each beneficiary who is reassigned, we compute her fit for every benchmark plan available in her market in her year of reassignment.

Our goal is to use this fit measure as a source of random variation in default plan value, and see how both consumption and active choice differ when fit changes. This requires that across-beneficiary variation in the assigned plan's fit is independent of other beneficiary-specific factors driving these outcomes. An *absolute* measure of fit (e.g., the share or number of drugs not covered) is a function of the number of drugs she was taking and is thus *not* necessarily independent of beneficiary-specific factors. Instead, for each beneficiary, we rank plans in the beneficiary's choice set and use the *within-beneficiary* quintile of fit for the beneficiary's assigned default plan, relative to other benchmark plans. The probability of being assigned a plan in any given quintile is $\frac{1}{5}$, which is independent of the beneficiary's characteristics by construction.

To estimate differences in the consequences of being assigned a poorly-fitting default versus a better-

fitting default, we replicate our stacked difference-in-differences design, but interact the indicator for a beneficiary's incumbent plan losing benchmark status with the fit quintile of their randomly-assigned default plan:

$$y_{itd} = \delta_t(LowestFit_{id} \times BenchmarkLoss_{id}) + \beta_t BenchmarkLoss_{id} + \gamma_{id} + \eta_{td} + \epsilon_{itd} \quad (3)$$

β_t represents the impact of being reassigned to a plan generally, while δ_t represents the *differential* effect of being assigned to a plan in the worst-fitting quintile. Random assignment of plans allows us to identify δ.

Figure 5b graphs event study estimates of $\beta_t + \delta_t$ (the total effect of being assigned a default plan in the worst-fitting quintile) and β_t (the effect of being assigned a default plan in any other quintile), with the outcome as the log of total allowed drug spending for a beneficiary in a given quarter. Beneficiaries have similar levels of drug spending prior to the default change. However, after their incumbent plan loses benchmark status, beneficiaries who face potential reassignment to the worst-fitting defaults see much larger spending reductions compared to those assigned better-fitting defaults.

Panel B of Table 3 presents the estimate pooling the pre- and post-treatment quarters. Re-assignment to the worst-fitting plans reduces drug spending by 12.6%, compared to a 4.3% effect of being reassigned to any other plan. Statistically and economically significant differences remain even as our price normalizations are applied.In Appendix C we show that these differences in consumption persist for at least *two years*, implying that beneficiaries are not offsetting this lost consumption over time by switching to drugs that *do* appear on the new plan's formulary or by switching to other plans with smaller consumption losses.

We can see how these results change as the variation widens by restricting to subsamples of beneficiaries who, due to their prior drug consumption, face greater differences in fit across benchmark plans. For each beneficiary, we measure the variance of fit in the benchmark plans available to them. We estimate the same regressions as above while progressively restricting to subsamples with the top 50%, 25%, and 10% of beneficiaries by variation in fit and report our estimates in the top panel of Table 6. As we restrict further, the consequences of default assignment to the worst-fitting plans worsen, with *total* consumption reductions (adding both the primary effect of reassignment and the effect of poor fit) of -24.9%, -27.6%, and -29.8%, respectively. The consequences of assignment to other plans also worsens (and so the difference between the worst-fitting plan and all others remains relatively stable around 20-22% of consumption), although this is driven by worsening consequences of all quintiles beside the very best. We report quintile-specific estimates for the main sample and each subsample in the 'Drug Response' column of Appendix Table A5. Reassuringly, these estimates are generally monotonic in the fit of the assigned plan.[25]

6.2 Active Choice Response

Assignment to default options with different 'fit' results in different drug consumption outcomes. Now, by seeing how active choice propensity changes in response to that assignment, we can measure $\frac{\partial a}{\partial v^d}$.

The assumption behind this strategy is that the drug consumption effects of a plan are a good proxy for the default plan's value to beneficiaries, v^d. This assumption is valid as long as our consumption measure is

[25] We provide estimates of spending effects using our drug- and class-level price normalizations in Appendix Table A6. We find substantial consumption effects despite these normalizations.

relevant for v^d (beneficiaries value consumption) and that it is not negatively correlated with other welfare-relevant characteristics of the default plan (plans that have better coverage are not worse on other dimensions, or at least not worse enough to offset the ordering implied by the fit measure). In the LIS program, plans have limited scope to vary benefits outside of formulary coverage, so a plan's fit should be the main point of differentiation with few other confounding factors.[26]

To measure the effect of default plan value on active choice, we re-estimate the event study regression from Equation 3, replacing the dependent variable with a measure of active choice. For beneficiaries whose default was to switch to a different plan, we define them as having made an active choice if they started a new enrollment spell with a code indicating that they made an active choice, or if they remained in their plan, which they only could have actively chosen to do. For beneficiaries whose default was to stay in their incumbent plan, we define them as having made an active choice if they switched into a different plan.[27] We measure this in cumulative terms in each quarter following the change in the default.

In Table 5, we present the results from such a regression, measuring whether the beneficiary made an active choice by December of the following year. While beneficiaries assigned to a plan in the worst-fitting quintile are slightly more likely to make an active choice relative to beneficiaries assigned to better-fitting plans, the difference is small (less than 2 percent of beneficiaries) and in both cases fewer than 7% of beneficiaries make an active choice. In Figure 6, we plot the quarter-level results from the event study regression.[28] The overall active choice rate barely changes as the year progresses, ruling out the possibility that beneficiaries assigned to poor-fitting plans learn about their mismatch over time and respond to it. In Figure A30 (in Appendix C) we show that this persists into the second year, and in Appendix B we show that it holds for an alternative model of fit where we weight drugs by price. Table 5 also shows (in the first column) similar results when the outcome is the propensity to switch plans at all (by active choice or not).

In Figure 7 we plot the one-year active opt-out propensity effects of assignment to each quintile of plan by fit against the estimated drug consumption losses from being assigned that plan. The slope is nearly flat. These results suggest that, despite experiencing *three times* the consumption losses from following a worse-fitting default, most beneficiaries nonetheless allow themselves to be enrolled in that default.

In the second panel of Table 6, we replicate our active choice propensity regressions, for the subsamples described in Section 4 where we restrict to beneficiaries with high variance in fit across potential benchmark plans. We see that even though the consequences of assignment to a poor-fitting plan rise sharply relative to our main sample, potentially facing nearly 30% reductions in drug consumption, the propensity to make an active choice barely rises. In Appendix Figures A15 and A16 we reproduce Figure 7 for the top 50% and 25% of beneficiaries by fit variance, with the underlying regressions in Appendix Table A5. Again, even beneficiaries facing exceptionally strong incentives do not appear to make an active choice in response.

[26] One remaining possibility is option value: While a plan might be a poor fit for present drugs, it might provide broad coverage for drugs the beneficiary might need in the future. However, for beneficiaries to value option value to the extent that they are willing to forgo substantial present consumption they would need an extraordinary level of risk aversion; moreover, since they can switch plans in any month, an attentive beneficiary can always reoptimize their plan relatively quickly if they face a new health event.

[27] We will mismeasure active choice for beneficiaries who actively stay in their assigned default plans. This will bias our estimates if beneficiaries are more likely to do this for plans of different fit. This relationship is likely to be positive (beneficiaries actively staying in better-fitting plans), suggesting that the true differential response is *even smaller* than our estimates.

[28] Pre-period values are fixed at zero since we have restricted our sample to beneficiaries who have remained in their plan before possible re-assignment.

These results suggest many beneficiaries give up substantial drug consumption by not making an active choice. This is a surprising non-response to a highly salient treatment: Unlike in the retirement savings context, here beneficiaries realize that a change has occurred when they arrive at the pharmacy and find that their prescriptions are no longer covered. Yet even *after* these changes are realized, they still do not opt out. Moreover, because the beneficiaries had experience in a prior plan, they can pay a small price (a few dollars per month) to return to that plan and regain full coverage of their previous prescriptions, incurring virtually no search costs. Yet almost no beneficiaries do this, no matter how damaging the new plan is in terms of fit and consumption losses. Even if beneficiaries have coarse information about their alternative options, we would expect beneficiaries who are more damaged by their current plan to exert more effort into learning about these options; yet they do not appear to do so.

Interpretation. Our results have two possible interpretations. The first, our preferred interpretation, is that the more accurate positive model of choice for beneficiaries is one in which default effects are driven by 'mental gaps,' and therefore changes in default value do little to influence active choice, either due to random inattention, or due to beneficiaries' knowledge of their choice environment being divorced from ground truth. That said, we cannot rule out the alternative interpretation that our setting is described by a 'frictional' model, but one in which the size of the choice friction varies across beneficiaries, and varies to a greater extent than the variation in default value induced by our natural experiment. Such a model can generate the above results, although our results put discipline on the plausible distribution of the friction.

To infer bounds on a beneficiary's frictions, we need to measure their foregone consumption from their passivity. We assume that beneficiaries facing reassignment would, if they made an active choice, return to their incumbent plan. This is a conservative assumption, as the beneficiary could potentially do better by searching for a free plan that covers their drugs sufficiently. We also need to map drug consumption to welfare. We assume that beneficiaries value drug consumption dollar-for-dollar. This is a strong assumption but maps cleanly into other more natural assumptions. For example, if we assume no moral hazard, which is typical in this literature, then the values from this assumption are a *lower* bound on value, as under no moral hazard, consumers of a drug value it at its price or greater. The presence of moral hazard or other sources of drug overconsumption will shade down our inference about consumed drugs' value.

In our results for beneficiaries in the top 10% of fit variation given in Appendix Table A5, we see that being assigned to the worst-fitting plan results in a loss of $1,112 of drug spending relative to those whose default is to remain in their incumbent plans. In contrast, those assigned to their best-fitting plan consume slightly more than they would in their incumbent plan, although this is indistinguishable from zero so we will assume no difference in consumption. The share of beneficiaries making an active choice, however, rises from 3.4% to only 7.6%, a difference of 4.2 percentage points. A beneficiary who returns to their incumbent plan must pay the nonzero premium. Our sample of beneficiaries were previously enrolled in incumbent plans where they would have had to pay, at most, $72/year ($6/month) to remain in the plan. Therefore, a beneficiary facing the worst possible default, and not opting out (the 92.4% who do not when assigned to the bottom fit quintile), must reveal themselves to have a friction of at least $1,040. In contrast, a beneficiary opting out of a good default (the 3.4% making an active choice in the top quintile) must have faced virtually no friction. Therefore, our results reveal that the distribution of frictions, if such a model

explains behavior, must be one in which 3.4% of beneficiaries have no friction, 4.2% have frictions between \$0 and \$1,040, and 92.4% of beneficiaries have frictions above \$1,040.[29]

The lower bound for the high-friction group is comparable in size to point estimates of average frictions found in the literature (Handel 2013, Handel and Kolstad 2015b, Polyakova 2016), and thus taking their approach would likely generate an even higher average friction. However, this setting represents an abnormally easy insurance choice problem relative to this prior work: Beneficiaries do not have to learn about complex plan features like deductibles and coinsurance rates since their covered spending is entirely subsidized, nor do they have to do any math to map their expected spending under a nonlinear contract. Therefore searching should be easier, making such large frictions counter-intuitive. Moreover, our results are only consistent with a highly bimodal distribution of frictions.

Ultimately, under a set of assumptions about beneficiary valuation of drug consumption, this exercise allows us to rule out large regions of the space of frictional models, with the remaining regions appearing unrealistic. We thus interpret our results as being more consistent with mental gap models of default effects, with the associated implications for the potential welfare consequences of passivity described in Section 5. Our results do, though, depend on our assumptions regarding the valuation of drug consumption. If beneficiaries value drugs at less than dollar-for-dollar, then our bounds will be weaker and more plausible under a frictional model.

7 Within-Beneficiary Variation in Active Choice

In the last section, we showed that beneficiary attention appears to be unresponsive to variation in the incentives to make an active choice. However, under some interpretations, that variation may be small. Therefore, in this section, we present an auxiliary test of the frictional model of default effects. The intuition is as follows: accepting both our prior results and a frictional model requires substantial across-beneficiary heterogeneity in frictions as an explanation. If beneficiaries face constant frictions over time, then almost all beneficiaries should fall into one of two categories: 'always choosers,' whose frictions are so low that they always make an active choice, or 'never choosers,' whose frictions are so high that no possible default should incentivize them to make a choice. If we were able to see sequences of potential decisions, our prior results suggest that we should primarily see 'always choosers' or 'never choosers,' with a small contingent of 'sometimes choosers.'

We test this hypothesis using sequential decisions. To observe sequential decisions, we use a new natural experiment. When a plan *exits the market* at the end of a year, *all* of its enrolled beneficiaries face an auto-assignment default, regardless of how they had previously enrolled in the plan. For these beneficiaries, we therefore have a sequence of two potential active choices: (1) their method of enrollment (active vs. passive) into their incumbent plan; and (2) their method of enrollment in their next plan after their incumbent plan's exit.[30]

[29] We can perform the same exercise for other samples. In our primary sample, 92.7% of beneficiaries must have frictions above \$324; in our top-50% sample, 91.2% of beneficiaries must have frictions above \$791, and in our top-25% sample, 91.7% of beneficiaries must have frictions above \$932.

[30] In contrast, if an LIS beneficiary actively enrolled in their incumbent plan, they are not eligible for the default reassignment mechanism we leveraged in Section 6, so we could not have used that setting to observe sequential active choice propensity.

We construct a subsample of all beneficiaries whose incumbent plan exited during our sample period. We restrict only to beneficiaries enrolled in exiting benchmark plans, since beneficiaries cannot passively enroll in non-benchmark plans. This gives us a sample of 84 exiting plans, with 32,852 incumbent beneficiaries, 28.3% of whom had previously actively enrolled in the exiting plan. Overall active choice is still very rare in this sample, with only 12.8% of beneficiaries making an active choice following their plan's exit.

A frictional model with constant frictions would imply that we should expect to see roughly 4.2% 'sometimes choosers,' and prior active choice status should nearly perfectly predict future active choice. Instead, we see 26.5% of our sample as 'sometimes choosers;' beneficiaries who either actively enrolled in their exiting plan or actively enrolled following their plan's exit, but *not* both. Prior passive auto-assignees make an active choice 7.7% of the time, whereas prior active choosers make an active choice only 25.6% of the time, a much higher rate but also one that is far less than 100%. In Table A7 we replicate this exercise in regression form, and extend it by controlling for demographic covariates, exiting plan fixed effects, and time since last choice. Sequentially adding these features improves the explanatory power of the regression (as measured by the R^2), but only to a small extent.

Structural Exercise. Our regressions above suggest that, in contrast to what we'd expect under a frictional model of choice, attention varies significantly *within beneficiaries* from year to year. How much does this within-beneficiary variation matter relative to the cross-beneficiary heterogeneity in frictions? To quantify their relative contributions, we consider a simple model of latent attention:

$$A_{it} = c_i + k_{it}$$

where c_i represents permanent, beneficiary-specific drivers of attention, and k_{it} represents i.i.d. transitory shocks that drive within-beneficiary variation. As in Section 5, beneficiaries make active choices if $A \geq 0$.

Our goal is to decompose the variance in A_{it} coming from variation in c across beneficiaries versus variation in k across time, while only observing a limited dependent variable which tells us if $A_{it} \geq 0$. In Appendix E we show that estimating this decomposition depends on the joint distribution of c_i and k_{it}. We assume that $c_i \sim \mathcal{N}(\mu X_i, \sigma^2)$ and $k_{it} \sim \mathcal{N}(0, 1)$. This allows for three potential sources of variation in attention: Cross-beneficiary permanent *observable* heterogeneity in X that drives differences in attention, with associated variance $\text{Var}(\mu X_i)$ (with unknown μ);[31] cross-beneficiary permanent *unobservable* heterogeneity in c_i, given by $\text{Var}(c_i|X)$, which has unknown variance σ^2; and variation in within-beneficiary transitory attention shocks k_{it} across time, whose variance we normalize to 1. We then fit this model to our data using maximum likelihood estimation. We describe this procedure in detail in Appendix E.

We present the results in Table 7. We estimate one model with no covariates (Column 1) and one with covariates (Column 2). For our covariates we include age (65-74 or 75+), gender (male or not), race (white or non-white), and Elixhauser Comorbidity Index (above or below median). We fully interact these characteristics to get 16 distinct groups and allow each to have a separate mean μ for c_i. We compute standard errors via bootstrap, using the same sample draws for both models.

[31] We measure all beneficiary observable heterogeneity at the time of plan exit and assume it is constant across time. Violations of this assumption will deflate our estimate of cross-beneficiary observable variation and inflate our estimate of transitory variation.

We find that about one-third of the variation in latent attention comes from permanent individual heterogeneity, whereas the other two-thirds comes from within-beneficiary transitory shocks. We find that heterogeneity in observable beneficiary characteristics explains only about one-tenth of the variation explained by unobservable beneficiary characteristics. What we observe is quite rich, though there are many important beneficiary characteristics we do not observe, such as education, (former) occupation, and socioeconomic status. Handel et al. (2020) show that such variables are important in explaining variation in insurance choice mistakes in the Netherlands, and indeed may be the source of this unobserved (in our data) heterogeneity. On the other hand, our results suggest that even a comprehensive dataset richer than what we have here would still be limited in its ability to predict beneficiary default behavior: Two-thirds of the variation occurs *within* beneficiaries across time.

Our results from this section suggest that the majority of variation in attention (and thus active choice) is driven by within-beneficiary transitory shocks, rather than cross-beneficiary variation in permanent frictions. This provides evidence against a story in which responses to default variation are low because heterogeneity in frictions is large. We can only rule out this explanation under the assumption that the frictions beneficiaries face are constant. While this is not an essential component of a frictional model,[32] in practice, empirical frictional models of default effects do have this property (Handel 2013, Polyakova 2016, Drake et al. 2022), likely because variation in frictions is difficult to identify separately from variation in preferences. We can also rule out some models described as mental gap models by Handel and Schwartzstein (2018), such as rational search with subjective prior beliefs, unless beliefs move idiosyncratically over time.

8 Conclusion

In this paper, we show that health insurance choices, and subsequent care utilization, are substantially driven by default rules. Most enrollees tend to follow default assignments, even when the default assignment leads to significant disruption of existing care patterns. Even these large disruptions do not trigger ex post reoptimization, with most beneficiaries waiting years to make an active plan choice, if they ever do. Although beneficiaries sporadically make active choices, this is not driven by incentives to do so, not even substantial incentives. While we can only make this conclusion for incentives within the domain we observe—that is, we cannot rule out that beneficiaries would make active choices if threatened with large fines, incarceration, or death—our setting includes a wide range of plausible market outcomes. Our setting focuses on patients who are older and sicker than average, and therefore may face higher cognitive costs, but, given the large frictions found in other settings, our results may plausibly generalize to other populations.

Our results largely reject the workhorse class of models used in much of the literature on default effects, where agents are endowed with a fixed friction stopping them from making an active choice, which they overcome if and only if the benefits of doing so are sufficiently high. Such a model cannot simultaneously explain why active choice varies significantly over time, yet does not respond to changes in material incentives. Candidate mechanisms must capture both of these facts.

Some mechanisms that do so are time-varying "mental gaps" such as random shocks to memory or

[32] E.g., in beta-delta models with infinite horizons, time-varying frictions help explain why beneficiaries do not procrastinate forever.

other external triggers. These triggers might include messaging from CMS, or new health events that make health insurance salient without necessarily having a large impact on the value of enrolling in any particular plan. Such a mechanism would explain other empirical patterns, such as the fact that LIS beneficiaries who make active choices typically make them during the enrollment period (when advertising and reminders are most common) even though they are not restricted to doing so. However, such triggers are not enough to explain the empirical pattern, as messaging from CMS (and other triggers, such as denials of coverage for prescriptions) are highly common, yet beneficiaries do not usually react to them with active choice. Therefore this pattern of external triggers must also be coupled with beneficiaries having misunderstandings about their choice environment, such as their agency over choices or what their alternative plan options are. For example, beneficiaries may not know they can switch plans in any month; they may not know they can switch at all, or may not understand that formularies vary across plans. Misunderstandings help explain the infrequency of even common heuristic responses to choice overload (e.g. reverting to one's previous plan when assigned to a poor-fitting default).

In the end, we cannot rule out *all* frictional mechanisms. One alternative frictional mechanism is one where beneficiaries face time-varying shocks to their friction, such as bouts of fatigue. However, to rationalize our findings, it must be the case that beneficiaries are commonly so fatigued as to ignore hundreds of dollars in equivalent drug consumption losses. Such a model is effectively indistinguishable from a model with completely random attention, and thus has similar welfare and policy implications.

Our results have important implications for the welfare consequences of passivity. First, passivity can have serious negative consequences for beneficiaries, both theoretically and empirically. If beneficiaries do not pay attention in response to poor circumstances, they may remain in those circumstances for arbitrarily long periods without altering them. Empirically, we observe this extreme attachment to harmful choices in practice: A significant share of beneficiaries remain in plans that have large negative effects on their drug consumption for arbitrarily long times. This suggests a role for 'paternalistic' defaults in the spirit of Thaler and Sunstein (2003), in which policymakers try to match beneficiaries to their best outcomes, rather than trying to incentivize active choice. Well-designed default rules can be a force that helps guarantee good outcomes for the inattentive, especially in complex settings like health insurance.

While it is clear that optimal default design should depend on the positive model of default effects, it is still unknown how other results in the literature might change under non-frictional models of attention. Such results include the idea that consumer response limits the extent of sellers' abilities to exploit inertia (Cabral 2016), or the idea that reducing behavioral biases can exacerbate adverse selection (Handel 2013). Our results suggest that there are more potential opportunities to explore how positive and normative results from behavioral economics depend on specific functional form assumptions.

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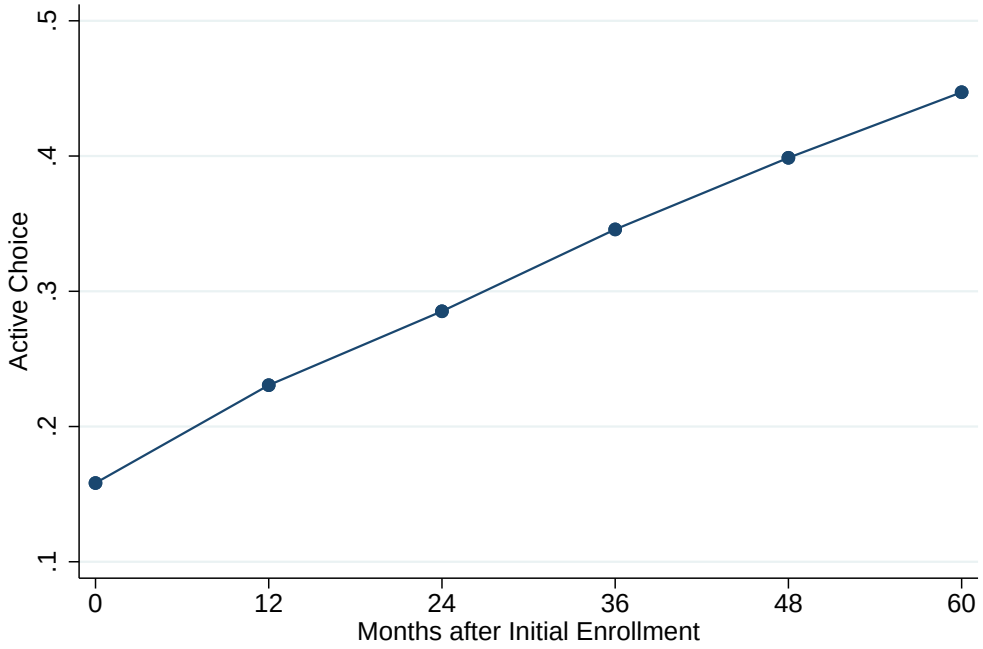
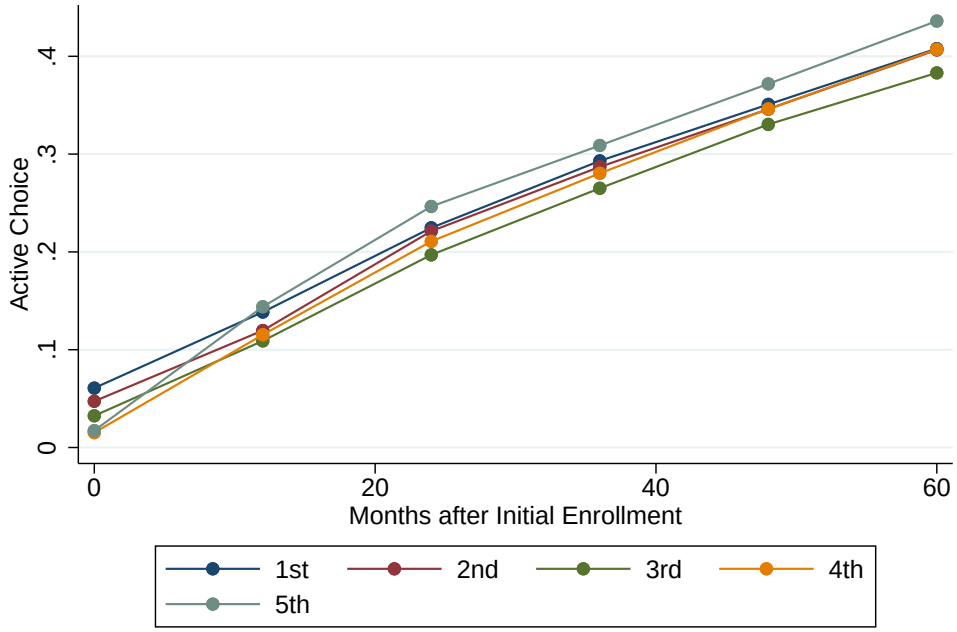
9 Figures

Figure 1: Cumulative Active Choice Rate among New Age-65 LIS Beneficiaries

(a) Full U.S. Sample

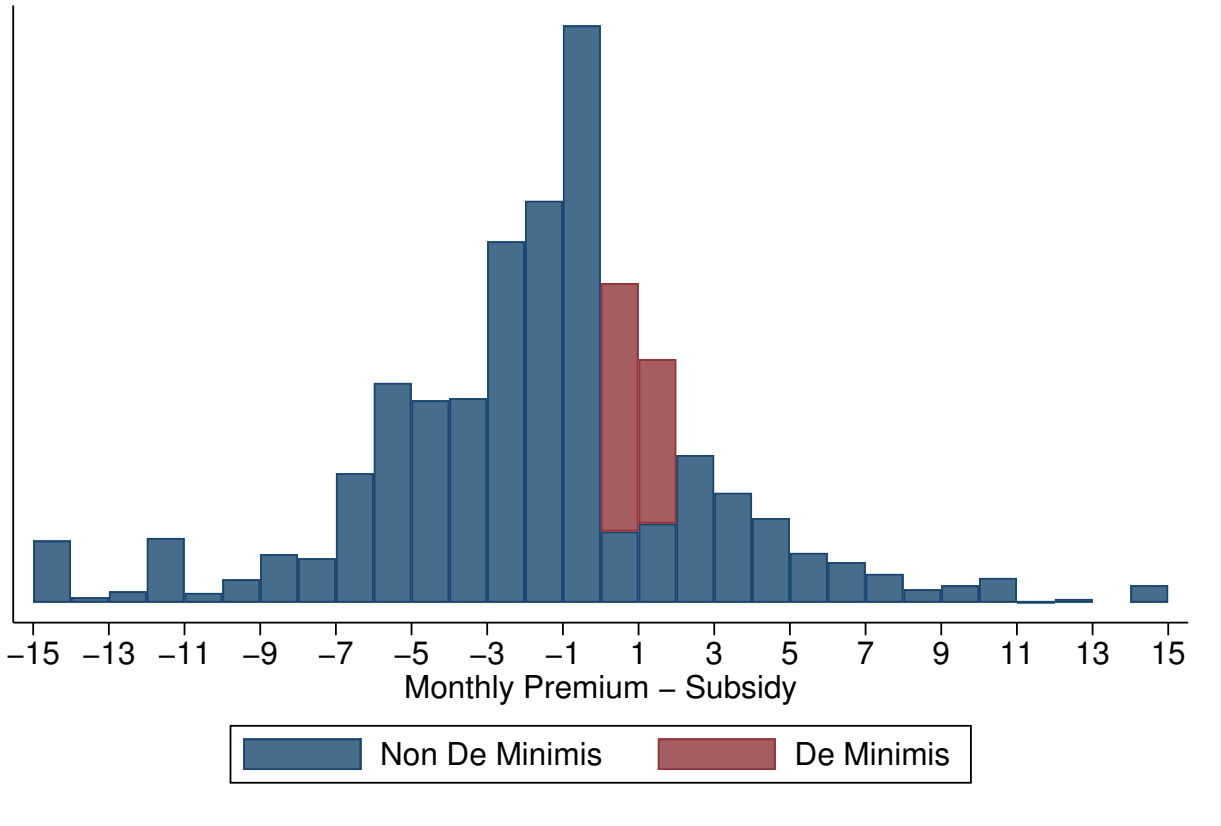


(b) Cumulative Active Choice, By Quintile of Fit



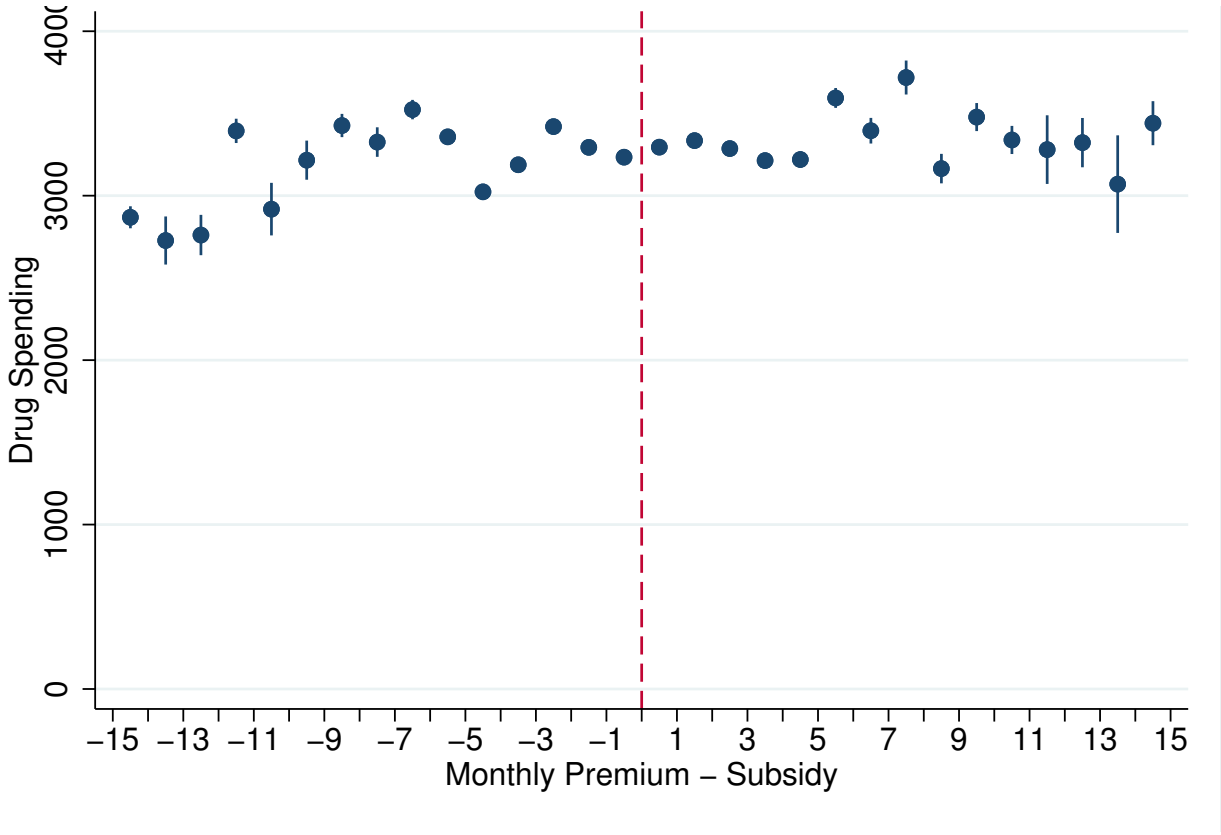
Notes: This figure plots the cumulative active choice propensity for LIS beneficiaries following their initial Medicare enrollment at age 65. Panel (a) is based on the sample of all LIS beneficiaries in the U.S. who turned 65 in 2007 - 2010. Panel (b) is based on the sample of LIS beneficiaries in the New York and Texas Medicaid-Medicare linked sample who turned 65 in 2007-2010. We split this sample into quintiles based on the rank of the plan they were assigned relative to other benchmark plans in terms of coverage of their drug consumption at age 64. Lower quintiles indicate better fit, with '1st' including beneficiaries assigned to the plans which best fit their previous drug consumption and '5th' indicating the worst-fitting plans. See Appendix B for more details on the plan fit measure, and Appendix Figure A1 for a figure that replicates Panel (a) using the subsample used in Panel (b).

Figure 2: Distribution of Difference between Plan Monthly Premium Bids and LIS Premium Subsidies



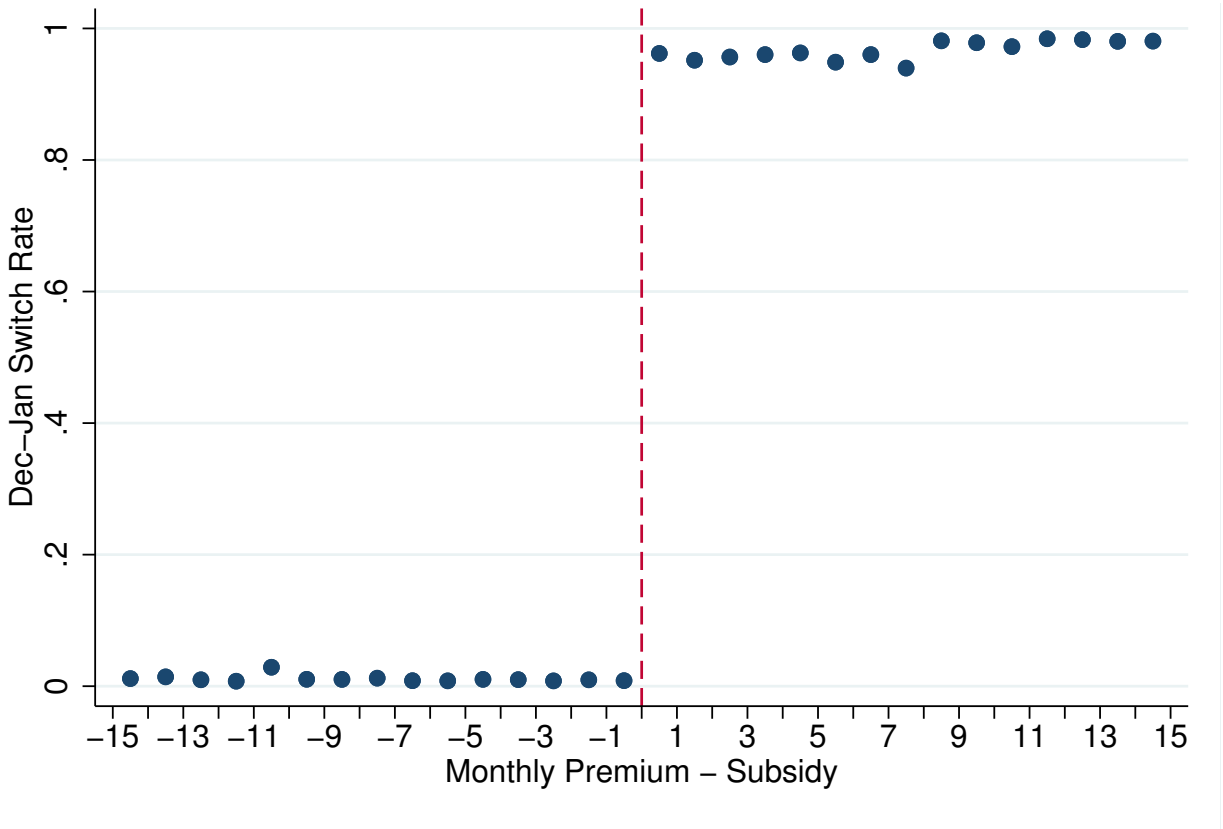
Notes: This figure plots the distribution of differences, in dollars, between a plan's bid for its monthly premium and the regional LIS premium subsidy in its region in that year. We restrict to plans that were benchmark plans in the prior year and weight plans by their enrollment in the prior year. We use the 'RD Analysis Sample' described in Table 1. The red mass indicates the share of beneficiaries enrolled in 'de minimis' plans that were allowed to retain their incumbent beneficiaries even though they bid above the subsidy amount. See Section 3.1 for more discussion of the 'de minimis' rules.

Figure 3: Regression Discontinuity Balance Tests for Prior Drug Spending



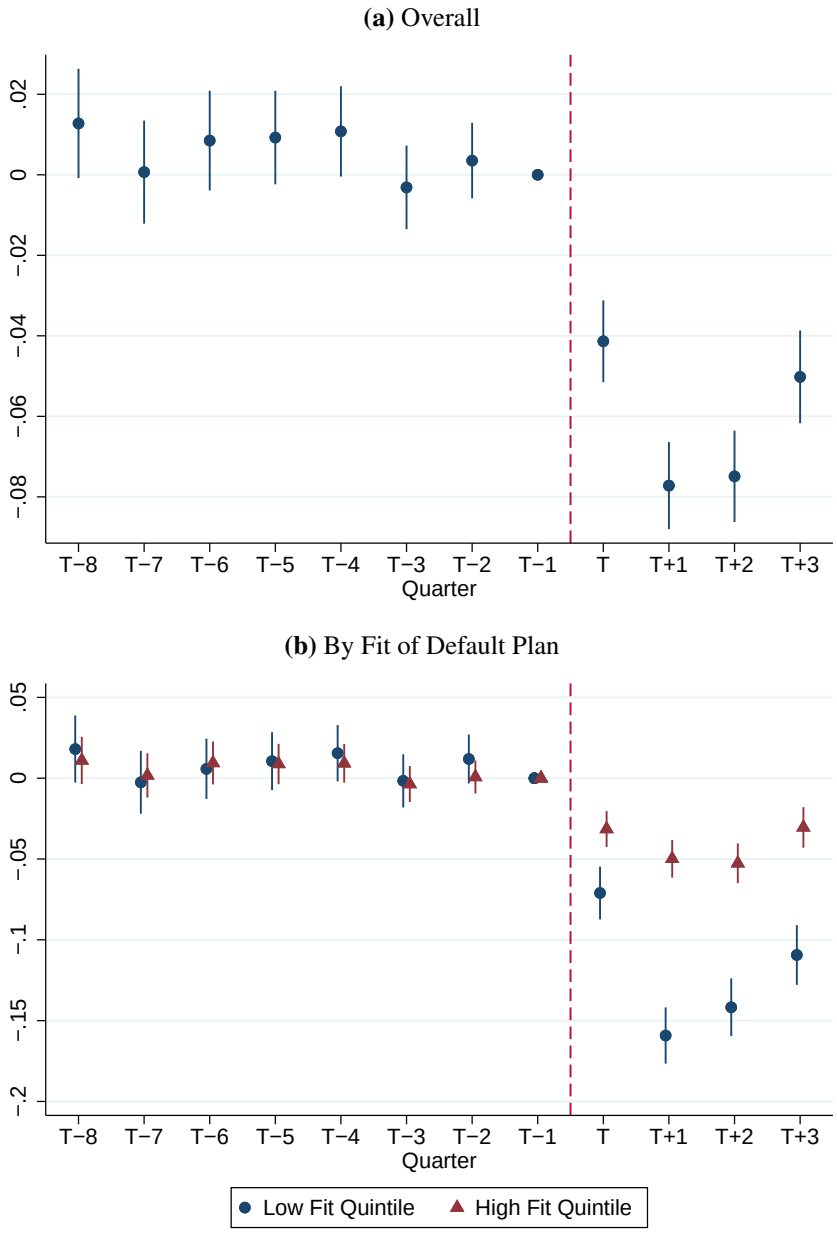
Notes: This figure plots average allowed prescription drug spending for beneficiaries in their prior year, for whole-dollar bins of the difference between their incumbent plans' monthly premium bid in the current year and the regional LIS premium subsidy in that year. We use the 'RD Analysis Sample' described in Table 1. In Appendix Figure A7 we replicate this figure for age, gender, non-drug medical spending, and the Elixhauser Comorbidity Index. Lines represent 95% confidence intervals. Points that appear to have no confidence intervals have intervals too small to be detected at the figure's scale.

Figure 4: Probability of Switching Plans, by Incumbent Plan Premium Relative to Local Market LIS Premium Subsidy



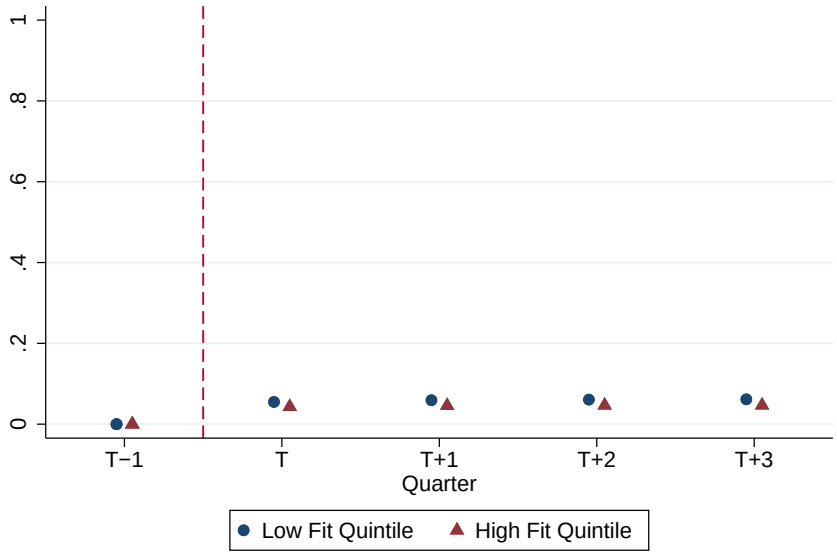
Notes: This figure plots the share of beneficiaries who switched plans, i.e., were enrolled in a plan in January that was different from the plan they were enrolled in during December of the previous year, for whole-dollar bins of the difference between their incumbent plans' monthly premium bid in the current year and the regional LIS premium subsidy in that year. We use the 'RD Analysis Sample' described in Table 1. 95% confidence intervals are too small to appear at the figure's scale.

Figure 5: Event Study Estimates of the Effect of Change in Default from Remaining in Incumbent Plan to Auto-Assignment to Randomly-Chosen Default Plan on Log Prescription Drug Spending



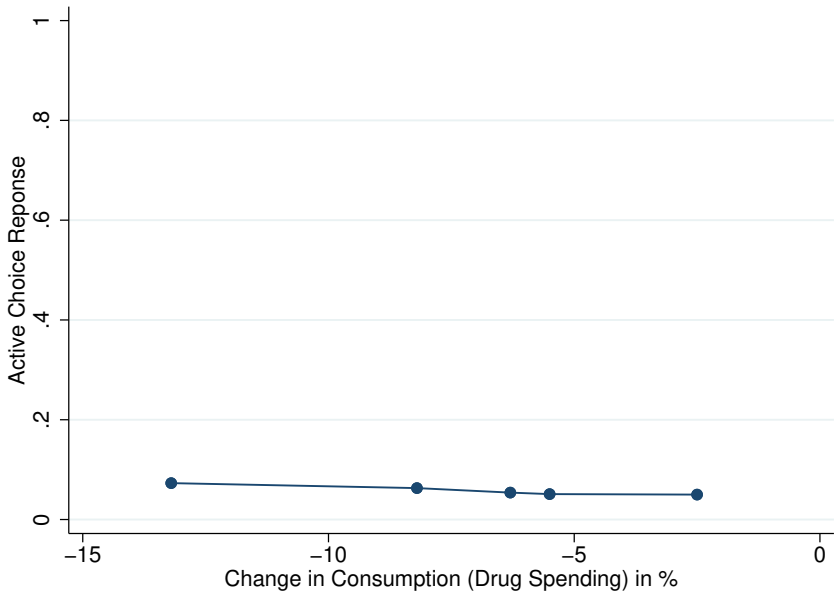
Notes: This figure plots event-study regression estimates of the effect of facing a default of reassignment to a randomly-chosen plan on quarterly drug spending outcomes, relative to facing a default of remaining in the incumbent plan. We use the 'RD Analysis Sample' described in Table 1 and estimate a stacked difference-in-differences design as described in Section 4. T represents the first quarter following potential reassignment through the default mechanism. The outcome variable is quarterly logged allowed prescription drug spending. In Panel (a), we estimate a single effect for all beneficiaries. For Panel (b), we allow for separate effects based on whether beneficiaries who faced a default of reassignment would have been assigned to a plan in the bottom quintile of plans they could have been assigned to, by the fit of that plans formulary for their own drug consumption. Red triangles represent treated beneficiaries assigned to a bottom-quintile plan, whereas blue triangles represent all other treated beneficiaries. Lines represent 95% confidence intervals.

Figure 6: Event Study Estimates of the Effect of Change in Default from Remaining in Incumbent Plan to Auto-Assignment to Randomly-Chosen Default Plan on Active Choice Propensity, by Fit of New Default Plan



Notes: This figure plots event-study regression estimates of the effect of facing a default of reassignment to a randomly-chosen plan on active choice propensity, relative to facing a default of remaining in the incumbent plan. We use the 'RD Analysis Sample' described in Table 1, and the model estimated is described in Section 6. The outcome is an indicator for whether the beneficiary had actively enrolled in the plan they were enrolled in at the end of the quarter. We allow for separate effects based on whether beneficiaries who faced a default of reassignment would have been assigned to a plan in the bottom quintile of plans they could have been assigned to, by the fit of that plans formulary for their own drug consumption. Red triangles represent treated beneficiaries assigned to a bottom-quintile plan, whereas blue triangles represent all other treated beneficiaries. 95% confidence intervals are too small to appear at the figure's scale.

Figure 7: Comparison of Default Effects on Drug Consumption Against Default Effects on Active Choice



Notes: This figure plots the relationship between the effect of default plan assignment on drug spending and the effect of default plan assignment on active choice propensity. To construct this figure, we estimate two difference-in-differences regressions of log drug spending and active choice propensity on whether the beneficiary faced a default of auto-assignment to a randomly-chosen plan, interacted with the 'fit' of that plans formulary for the beneficiary's drug consumption, binned into quintiles. Each dot represents a pair of quintile-specific spending (measured in percent changed) and active choice effects. The regressions underlying this figure are described in Sections 4 and 6, and the specific estimates are given in Appendix Table A5.

10 Tables

Table 1: Summary statistics for samples used in our analysis.

	Broad LIS Sample	National MCR Initial Enrollment Sample	NY TX MCD-MCR Linked Sample	RD Analysis Sample
Avg. Age	77.2	66	66	76.6
% Female	69.5%	62.2%	62.2%	69.1%
Avg. Elixhauser Index	4.5	3.4	4.4	4.2
Total Drug Spending				
Mean	$4,024	$3,007	$4,231	$3,707
25th Percentile	$815	$298	$871	$710
Median	$2,601	$1,561	$2,661	$2,384
75th Percentile	$5,389	$3,945	$5,364	$5,021
Avg. Chronic Drug Spending	$3,012	$2,264	$3,230	$2,821
Avg. Non-Drug Med. Spending	$12,888	$8,413	$11,329	$10,945
Observations	4,628,704	216,772	14,218	1,989,603

Notes: The table includes summary statistics for four samples. For all of our analyses in this paper, we employ a set of common restrictions in the samples as described in Section 1.2. In this table we restrict to the 20% subsample for which we can observe data on medical and drug utilization. The "Broad LIS Sample" reflects that full sample. The "National MCR Initial Enrollment Sample" restricts to the first year of beneficiaries' Medicare enrollment (at age 65). The "NY TX MCD-Linked Sample" further restricts to beneficiaries residing in New York and Texas who were on Medicaid due to disability prior to enrolling in Medicare at age 65. The "RD Analysis Sample" includes LIS beneficiaries who were enrolled in a benchmark Part D plan (in year $t-1$) and continued to enroll in Medicare in the following year t. The Elixhauser Index is a count of co-morbidities, calculated from all Medicare inpatient, outpatient, and carrier files in the corresponding beneficiary-year for the "Broad LIS Sample", in first calendar year post initial Medicare enrollment for the "National MCR Initial Enrollment Sample" and "NY TX MCD-Linked Sample", and in year t-1 for the "RD Analysis Sample". Meanwhile, the spending measures are based on Medicare data in the corresponding beneficiary-year for the "Broad LIS Sample", in first calendar year post initial Medicare enrollment for the "National MCR Initial Enrollment Sample" and "NY TX MCD-Linked Sample", and in year t for the "RD Analysis Sample". Chronic drugs are defined as NDC codes where the median number of fills per person in a year is greater than two, among the subset of people with at least one fill.

Table 2: Regression Discontinuity Estimates of the Effects of a Change in Default from Remaining in Incumbent Plan to Auto-assignment to Randomly-Selected Default Plan

	Outcomes: Switched Plans			
	Switched	Switched By Active Choice	Switched to Benchmark Plan By Active Choice	Switched By Default Reassignment
Beneficiary Faced Default of Plan Reassignment	0.960 (0.001)	0.024 (0.001)	0.020 (0.001)	0.936 (0.001)
Average Value for Control Beneficaries	0.006	0.006	0.004	0
Observations			460,729	

Notes: This table reports regression discontinuity estimates of the effect of facing a default of reassignment to a randomly-chosen plan on the probability of switching plans, relative to facing a default of remaining in the incumbent plan. The reported coefficient is for a dummy variable that indicates whether a beneficiary's incumbent plan set a monthly premium bid greater than the LIS premium subsidy in that region and year. All regressions use the 'RD Analysis Sample' in Table 1, restricted to beneficiaries whose incumbent plans bid within $6 of the subsidy, and include region-year fixed effects. Each column reflects a regression with a different dependent variable. The first column's dependent variable is an indicator for whether the beneficiary switched plans, i.e., was enrolled in a different plan in January of the current year than they were in December of the prior year. The second's is an indicator for whether the beneficiary switched plans by actively enrolling in a different plan. The third's is an indicator for whether the beneficiary actively switched to a benchmark plan. The fourth's is an indicator for whether the beneficiary was reassigned to a different plan by default rather than by active choice.

Table 3: Difference-in-Difference Regression Estimates of the Effects of a Change in Default from Remaining in Incumbent Plan to Auto-assignment to Randomly-Selected Default Plan on Drug Consumption

	Outcomes: Log(Drug Spending)		
	-	With Drug-Level Price Normalization	With Class-Level Price Normalization
Panel A			
Faced Plan Reassignment Default × Post	-0.064	-0.050	-0.021
	(0.004)	(0.004)	(0.004)
Panel B			
Faced Plan Reassignment Default × Post	-0.043	-0.030	-0.012
	(0.004)	(0.004)	(0.004)
Faced Plan Reassignment Default × Assigned Plan in Worst Quintile by Fit × Post	-0.083	-0.082	-0.036
	(0.007)	(0.007)	(0.006)
Average Spending for Control Beneficiaries in Prior Year	$3,329	$3,412	$3,503
Observations		5,574,684	

Notes: This table reports difference-in-differences regression estimates of the effect of facing a default of reassignment to a randomly-chosen plan on quarterly drug spending outcomes, relative to facing a default of remaining in the incumbent plan. All regressions use the 'RD Analysis Sample' in Table 1, restricted to beneficiaries whose incumbent plans bid within $6 of the subsidy, and include beneficiary-experiment and experiment-quarter fixed effects, where an 'experiment' is defined as a cohort-region pair as described in Section 4. In Panel A, we report the coefficient for a regression where the primary treatment variable is a dummy variable that indicates whether a beneficiary's incumbent plan set a monthly premium bid greater than the LIS premium subsidy in that region and the 'post' year, interacted with whether the observation was in the 'post' year. In Panel B, we report the coefficients for an alternative regression where we additionally interact the treatment with an indicator for whether the beneficiary, if they faced a default of reassignment, would be assigned to a plan in the bottom quintile of plans they could have been assigned to, by the 'fit' of that plan's formulary for their own drug consumption. In the first column, the dependent variable is logged allowed spending on all drugs during the year. In the second column, we normalize the prices for all drugs by the average price for their NDC across all plans and years before computing spending. In the third column, we normalize prices by the average price of all drugs in the same therapeutic class across all plans and years.

Table 4: Difference-in-Difference Regression Estimates of the Effects of a Change in Default from Remaining in Incumbent Plan to Auto-assignment to Randomly-Selected Default Plan on Additional Drug Consumption Outcomes

	Outcome: Log(Drug Spending)		
	High-Value	Chronic	Non-Chronic
Panel A			
Faced Plan Reassignment Default	-0.062	-0.078	-0.028
× Post	(0.005)	(0.005)	(0.006)
Panel B			
Faced Plan Reassignment Default	-0.049	-0.057	-0.022
× Post	(0.005)	(0.005)	(0.006)
Faced Plan Reassignment Default	-0.052	-0.085	-0.023
× Assigned Plan in Worst Quintile by Fit	(0.007)	(0.008)	(0.009)
× Post			
Average Spending for Control Beneficiaries in Prior Year	$1,237	$2,496	$834
Observations		5,574,684	

Notes: This table reports difference-in-differences regression estimates of the effect of facing a default of reassignment to a randomly-chosen plan on quarterly drug spending outcomes, relative to facing a default of remaining in the incumbent plan. All regressions use the 'RD Analysis Sample' in Table 1, restricted to beneficiaries whose incumbent plans bid within $6 of the subsidy, and include beneficiary-experiment and experiment-quarter fixed effects, where an 'experiment' is defined as a cohort-region pair as described in Section 4. In Panel A, we report the coefficient for a regression where the primary treatment variable is a dummy variable that indicates whether a beneficiary's incumbent plan set a monthly premium bid greater than the LIS premium subsidy in that region and the 'post' year, interacted with whether the observation was in the 'post' year. In Panel B, we report the coefficients for an alternative regression where we additionally interact the treatment with an indicator for whether the beneficiary, if they faced a default of reassignment, would be assigned to a plan in the bottom quintile of plans they could have been assigned to, by the 'fit' of that plan's formulary for their own drug consumption. In the first column, the dependent variable is logged allowed spending on "high-value" drugs during the year, where high-value is defined in Section 4. In the second column, the dependent variable is logged allowed spending on chronic drugs, which are drugs for which the median number of annual, per person fills is greater than two, among the subset of people with at least one annual fill. In the third column the dependent variable is logged allowed spending on non-chronic drugs, which are any drugs not classified as chronic drugs.

Table 5: Difference-in-Difference Regression Estimates of the Effects of a Change in Default from Remaining in Incumbent Plan to Auto-assignment to Randomly-Selected Default Plan on Switching and Active Choice

	Switched From Incumbent Plan by December	Active Choice by December
Panel A		
Faced Plan Reassignment Default	0.955	0.050
	(0.001)	(0.001)
Panel B		
Faced Plan Reassignment Default	0.955	0.047
	(0.001)	(0.001)
Faced Plan Reassignment Default	0.002	0.015
$\times$ Assigned Plan in Worst Quintile by Fit	(0.002)	(0.002)
Average Value for Control Beneficiaries	0.011	0.010
Observations	460,729	

Notes: This table reports difference-in-difference regression estimates of the effect of facing a default of reassignment to a randomly-chosen plan on switching and active choice outcomes, relative to facing a default of remaining in the incumbent plan. All regressions use the 'RD Analysis Sample' in Table 1, restricted to beneficiaries whose incumbent plans bid within $6 of the subsidy, and include region-year fixed effects. In Panel A, we report the coefficient for a regression where the primary treatment variable is a dummy variable that indicates whether a beneficiary's incumbent plan set a monthly premium bid greater than the LIS premium subsidy in that region and year. In Panel B, we report the coefficients for an alternative regression where we additionally interact the treatment with an indicator for whether the beneficiary, if they faced a default of reassignment, would be assigned to a plan in the bottom quintile of plans they could have been assigned to, by the 'fit' of that plan's formulary for their own drug consumption. In the first column, the outcome is an indicator for whether a beneficiary was enrolled in a different plan in December of that year than the plan they enrolled in during the prior year. In the second column, the outcome is a binary indicator for whether a beneficiary had made an active choice to enroll in the plan they were in as of December of that year.

Table 6: Effects of Plan Assignment on Spending and Active Choice for Beneficiaries With Large Variance in Fit Across Plans

		Subsample (Quantile of Variance in Fit)		
	All	Top 50th	Top 25th	Top 10th
		Outcome: Spending		
Faced Plan Reassignment Default	-0.043	-0.049	-0.055	-0.087
× Post	(0.004)	(0.006)	(0.008)	(0.014)
Faced Plan Reassignment Default	-0.083	-0.200	-0.222	-0.211
× Assigned Plan in Worst Quintile by Fit	(0.007)	(0.010)	(0.015)	(0.026)
× Post				
		Outcome: Active Choice by December		
Faced Plan Reassignment Default	0.047	0.054	0.049	0.041
	(0.001)	(0.001)	(0.002)	(0.003)
Faced Plan Reassignment Default	0.016	0.034	0.034	0.037
× Assigned Plan in Worst Quintile by Fit	(0.002)	(0.003)	(0.004)	(0.006)

Notes: This table reports results from regressions in which we replicate the specifications from the first column of Table 3, Panel B, and the second column of Table 5, restricted to subsamples. We define these subsamples based on the variance of 'fit' across benchmark plans in that beneficiary's region and year. The first column directly replicates the results from the prior tables. In the second through fourth columns, we restrict to the top 50%, 25%, and 10% of beneficiaries, ranked by this variance measure, respectively.

Table 7: Parameter Estimates from Structural Model of Active Choice

	(1)	(2)
Variance Component	**Share of Variance in Latent Attention Explained**	
Variation in Across-Beneficiary Observable Characteristics ($\text{Var}[\mu X_i]$)	-	3.2% [2.9%, 3.8%]
Variation in Across-Beneficiary Unobservable Characteristics ($\text{Var}[c_i \mid X_i] = \sigma^2$)	33.4% [31.4%, 35.4%]	30.6% [28.7%, 32.5%]
Variation in Within-Beneficiary Transitory Attention Shocks ($\text{Var}[k_{it}]$)	66.6% [64.6%, 68.6%]	66.2% [64.1%, 68.0%]
Average Latent Attention ($E[\mu X_i]$)	-1.01 [-1.03,-0.99]	-1.01 [-1.03, -0.99]
Standard Deviation of Beneficiary-Specific Unobservable Latent Attention Component (σ)	0.71 [0.68,0.74]	0.68 [0.65,0.71]
Observations	32,852	

Notes: This table reports outcomes derived from estimates from a structural parametric model of attention described in Section 7. In the model, latent attention A_{it} is modeled as $A_{it} = c_i + k_{it}$, with $c_i \sim \mathcal{N}(\mu X_i, \sigma^2)$ and $k_i \sim \mathcal{N}(0, 1)$ for beneficiary observable characteristics X_i. The first column presents estimates from a model in which $X_i = 1$ for all i, whereas the second column presents estimates from a model where X_i includes bins of age, gender, race, and the Elixhauser Comorbidity index. The upper panel of this table reports outcomes a variance decomposition exercise where we report the share of variance in latent attention A explained by variation in three factors: Across-beneficiary observable characteristics X_i, across-beneficiary unobservable characteristics, and within-beneficiary, across-time shocks to attention. The share of variance for is computed by dividing the variance from that component by the sum of variance across all components. The lower panel of this table reports the average across-beneficiary latent attention factor, as well as the standard deviation in the unobservable across beneficiary component, estimated as σ. Both of these are measured relative to a one-standard-deviation change in the transitory attention shock k_{it}. Below each estimate we give the 95% confidence interval generated from 1000 bootstrap runs.

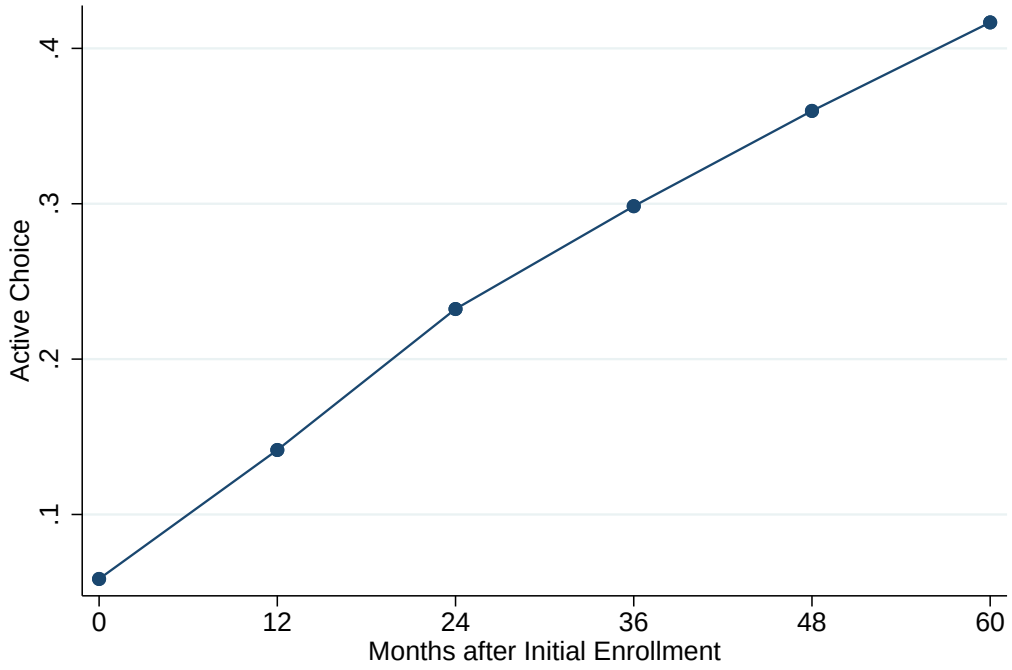
For Online Publication

Appendix for:

The Behavioral Foundations of Default Effects: Theory and Evidence from Medicare Part D

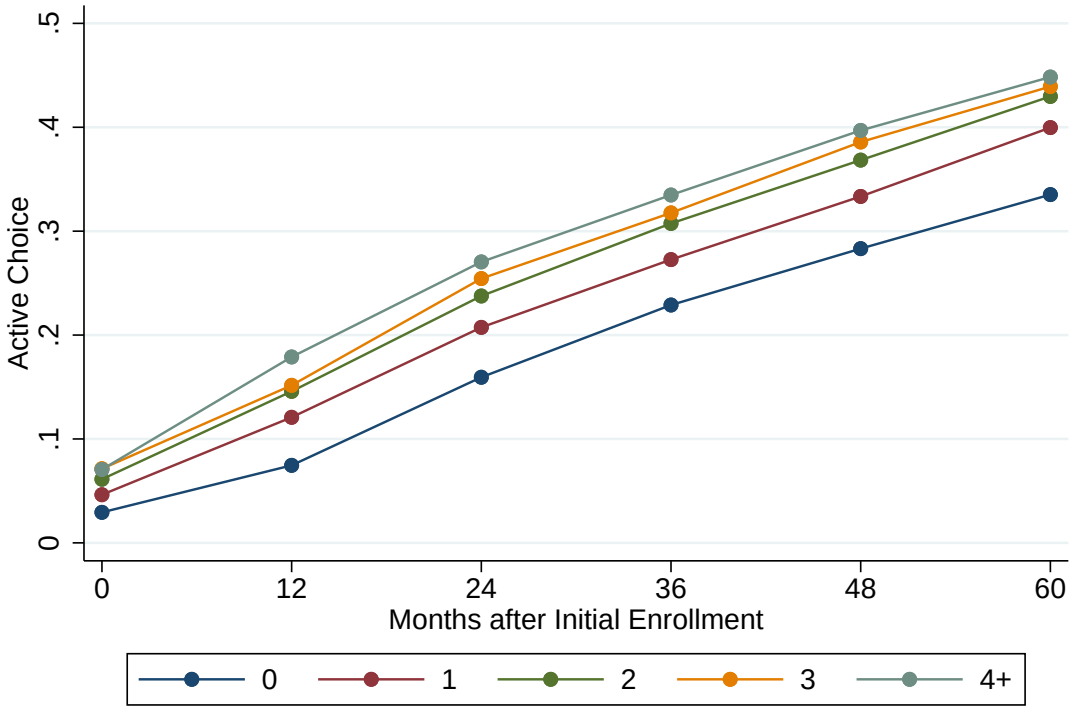
A Appendix: Additional Figures and Tables

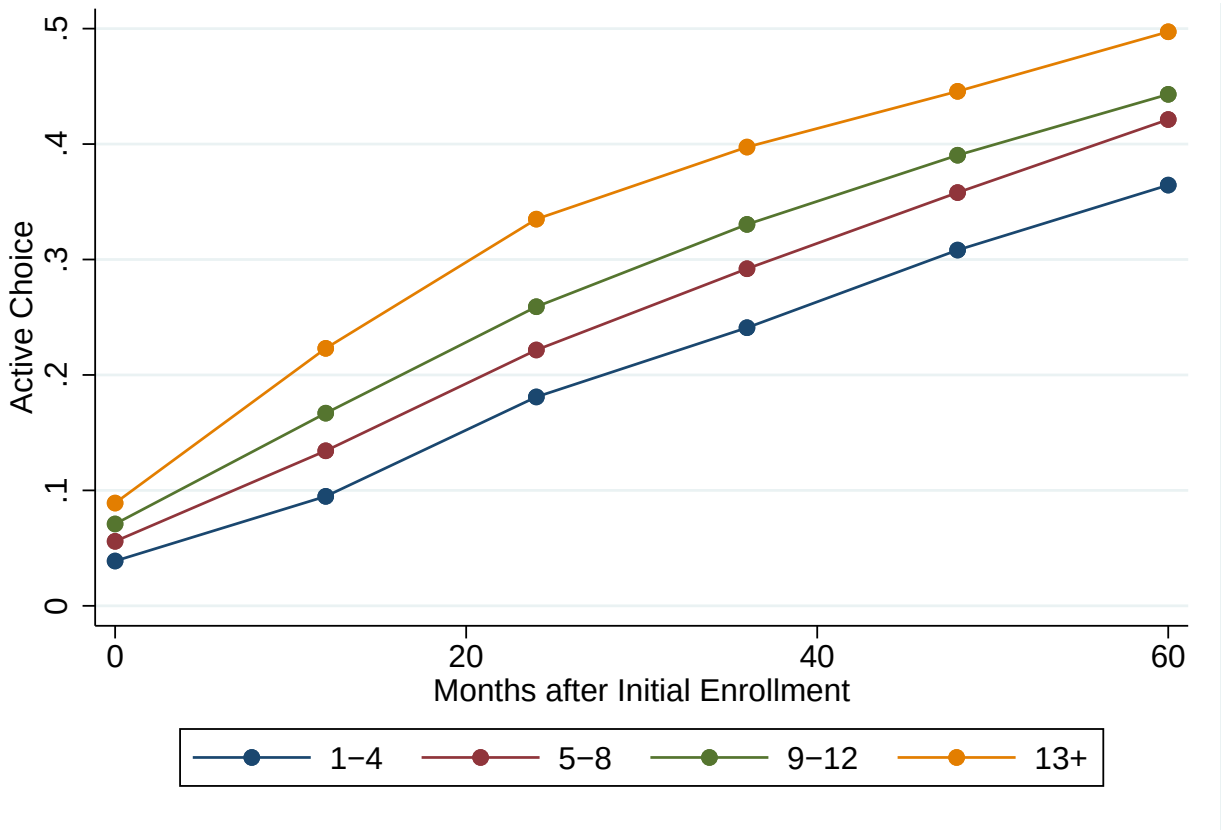
Appendix Figure A1: NY and TX Medicaid-Linked Sample



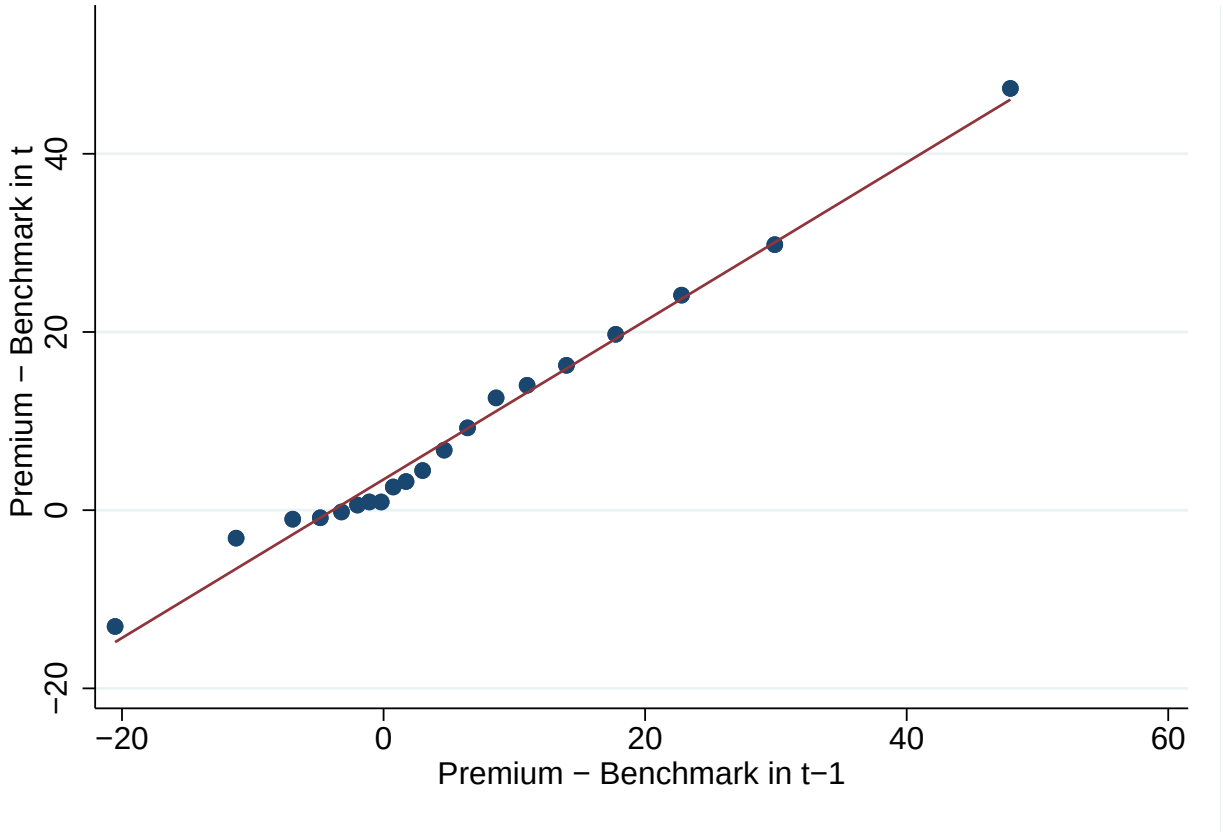
Notes: Figure plots the cumulative active choice propensity for LIS beneficiaries following their initial Medicare enrollment (at age 65). Panel (a) is based on the sample of all LIS beneficiaries in the U.S. who turned 65 in 2007 - 2010. Panel (b) is based on a subsample of those living in New York and Texas, and who were enrolled in Medicaid at age 64 due to disability before enrolling in Medicare.

Appendix Figure A2: Cumulative Active Choice Propensity among New Age-65 LIS Beneficiaries By Elixhauser Comorbidity Index, Measured at Age 64

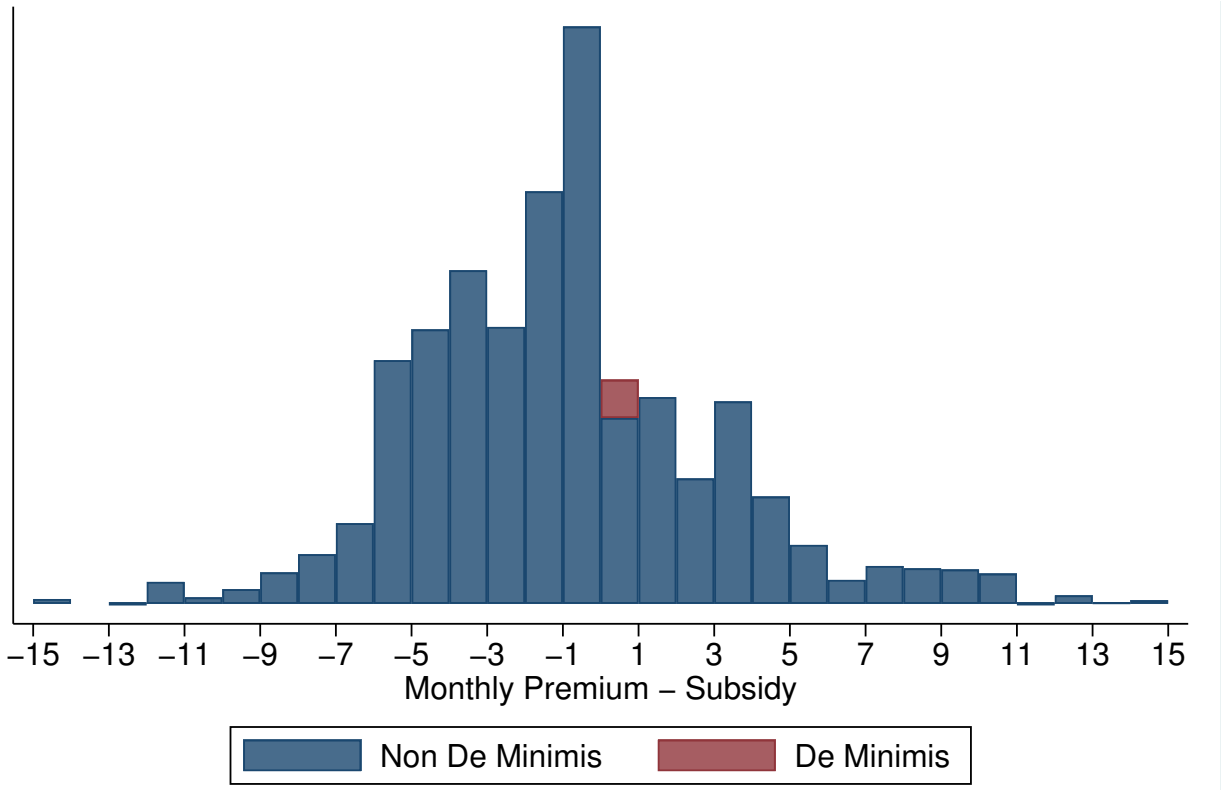




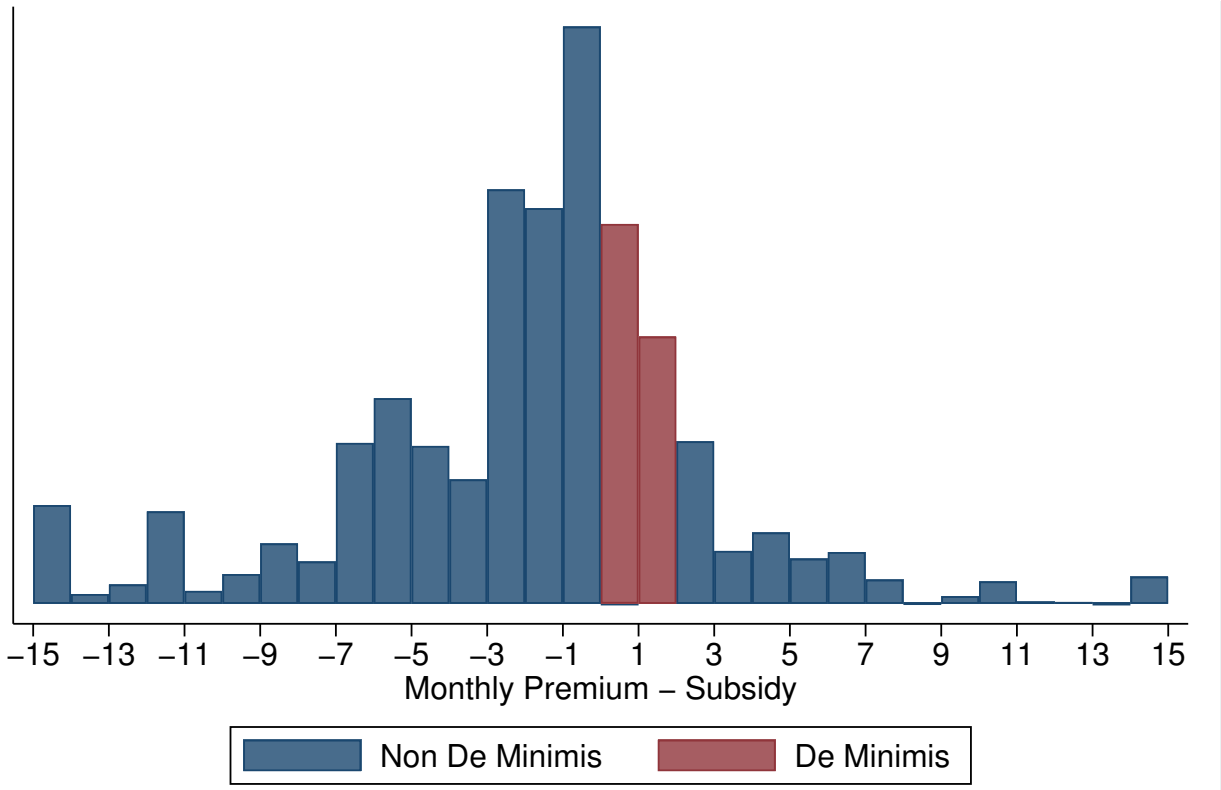
Appendix Figure A3: Cumulative active choice propensity for our Medicaid-linked sample by number of unique drugs taken at 64.



Appendix Figure A4: This figure plots premiums in $t-1$ against premiums in t for all plans in our data that existed in consecutive year pairs.

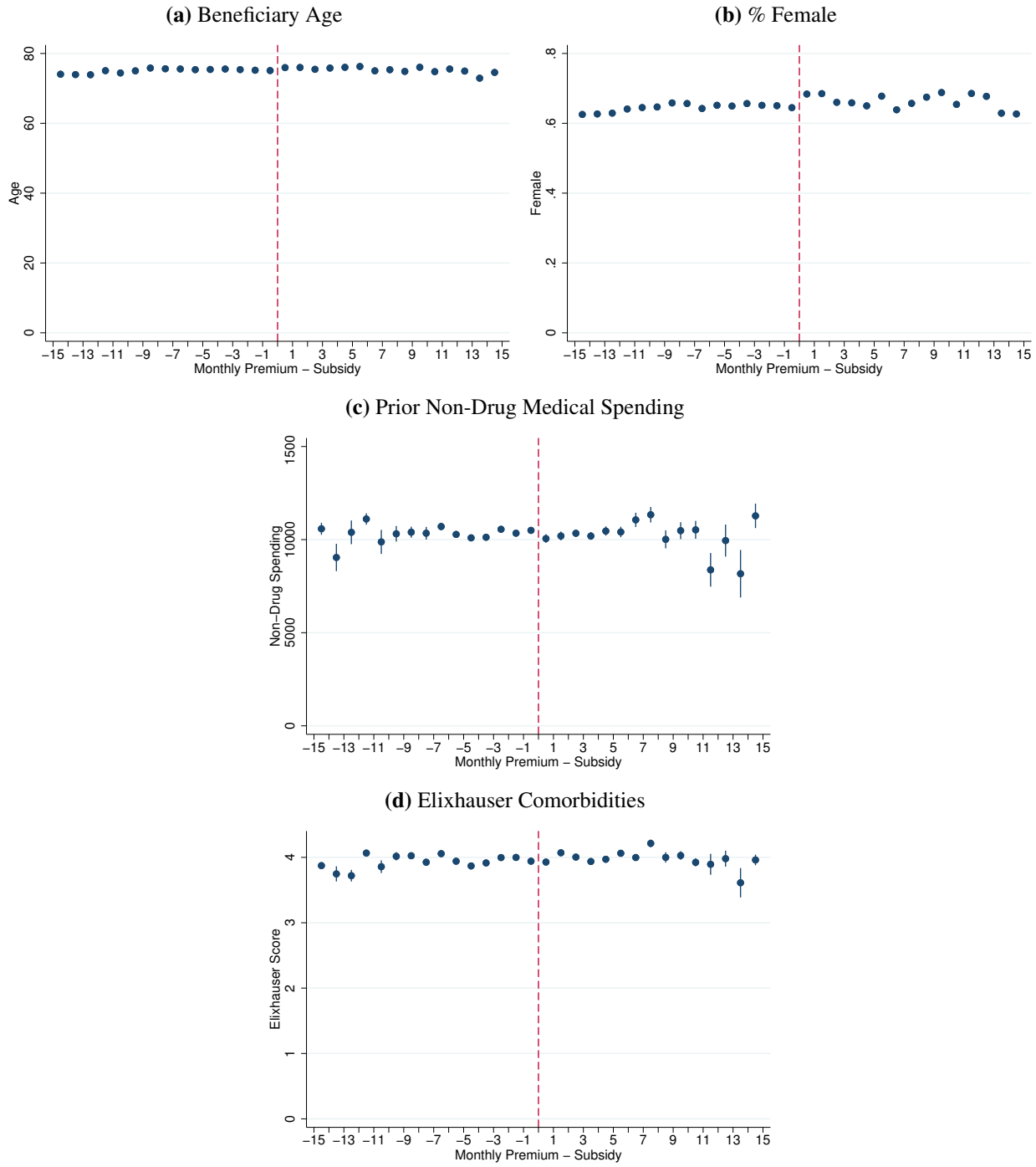


Appendix Figure A5: This figure replicates Figure 2, restricting only to 2007-2010, before the de minimis rules were implemented.

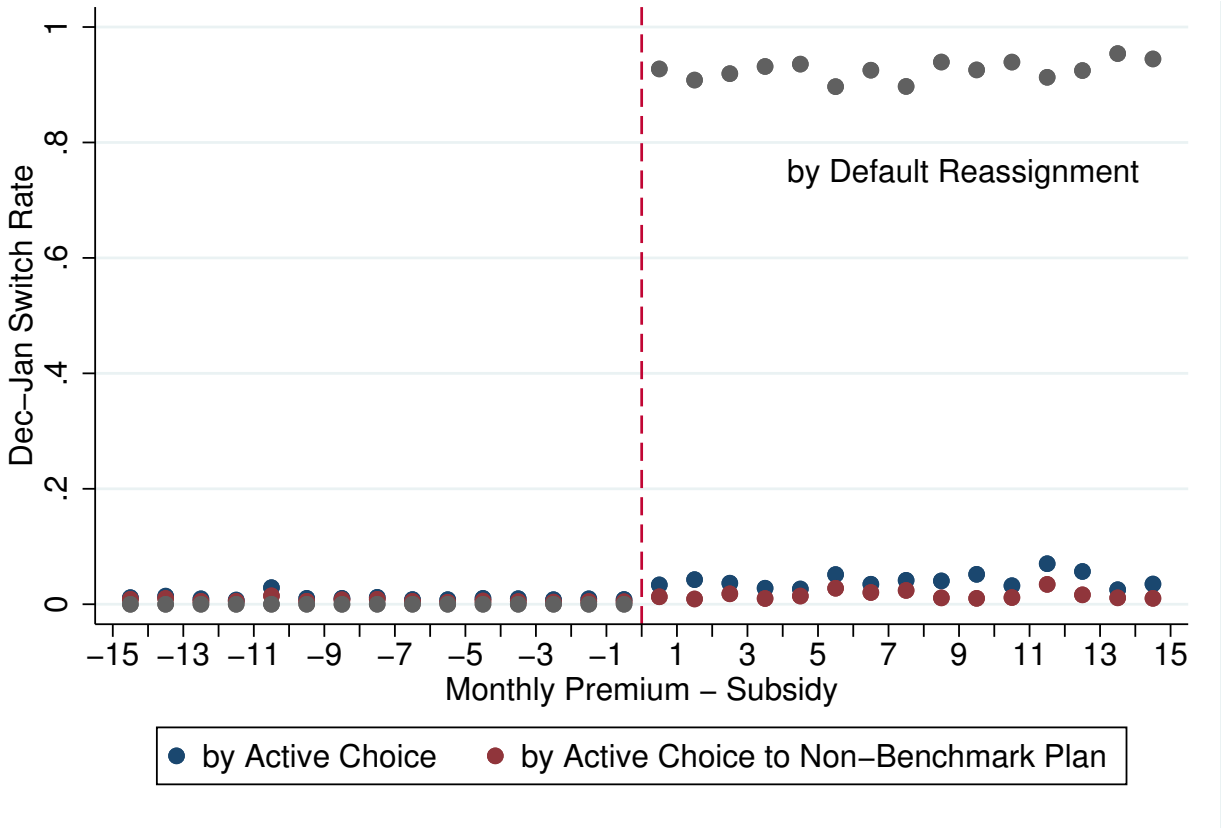


Appendix Figure A6: This figure replicates Figure 2, restricting only to 2011 onwards, after the de minimis rules were implemented.

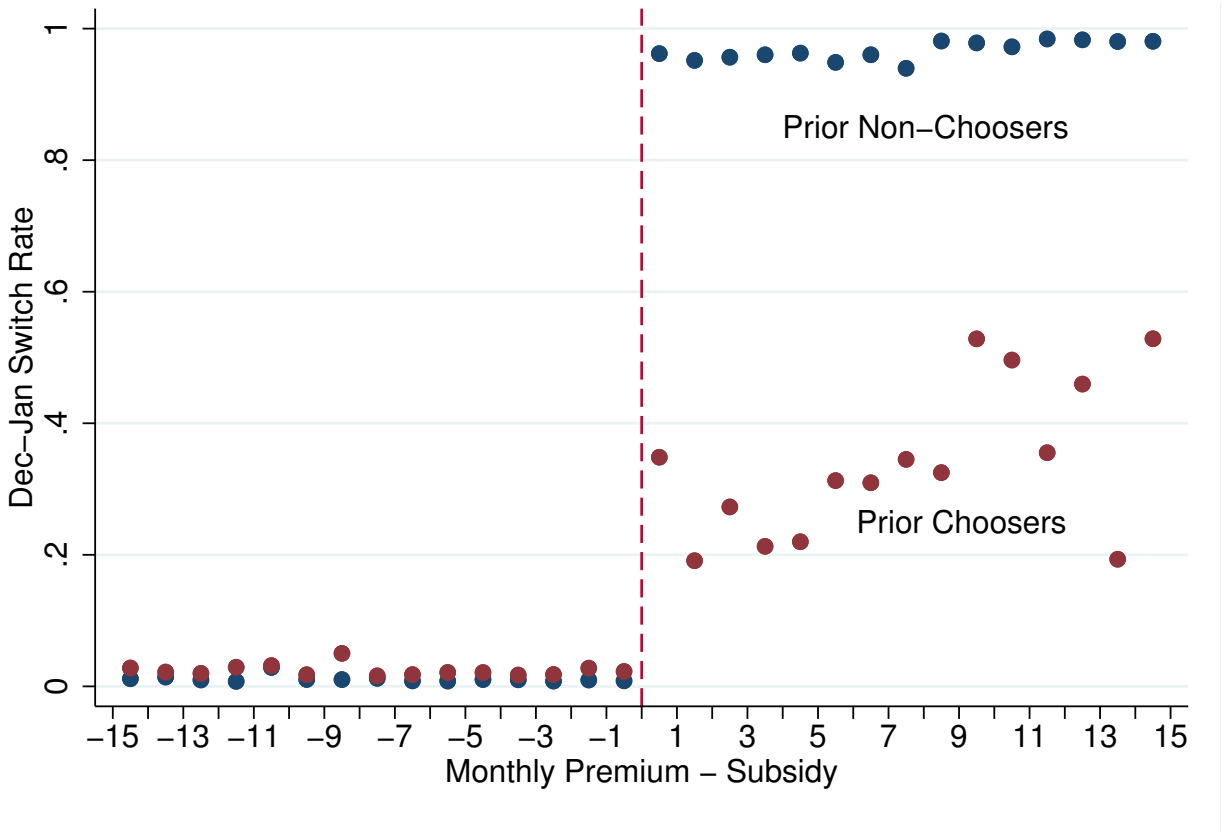
Appendix Figure A7: Regression Discontinuity Balance Tests for Observable Beneficiary Characteristics



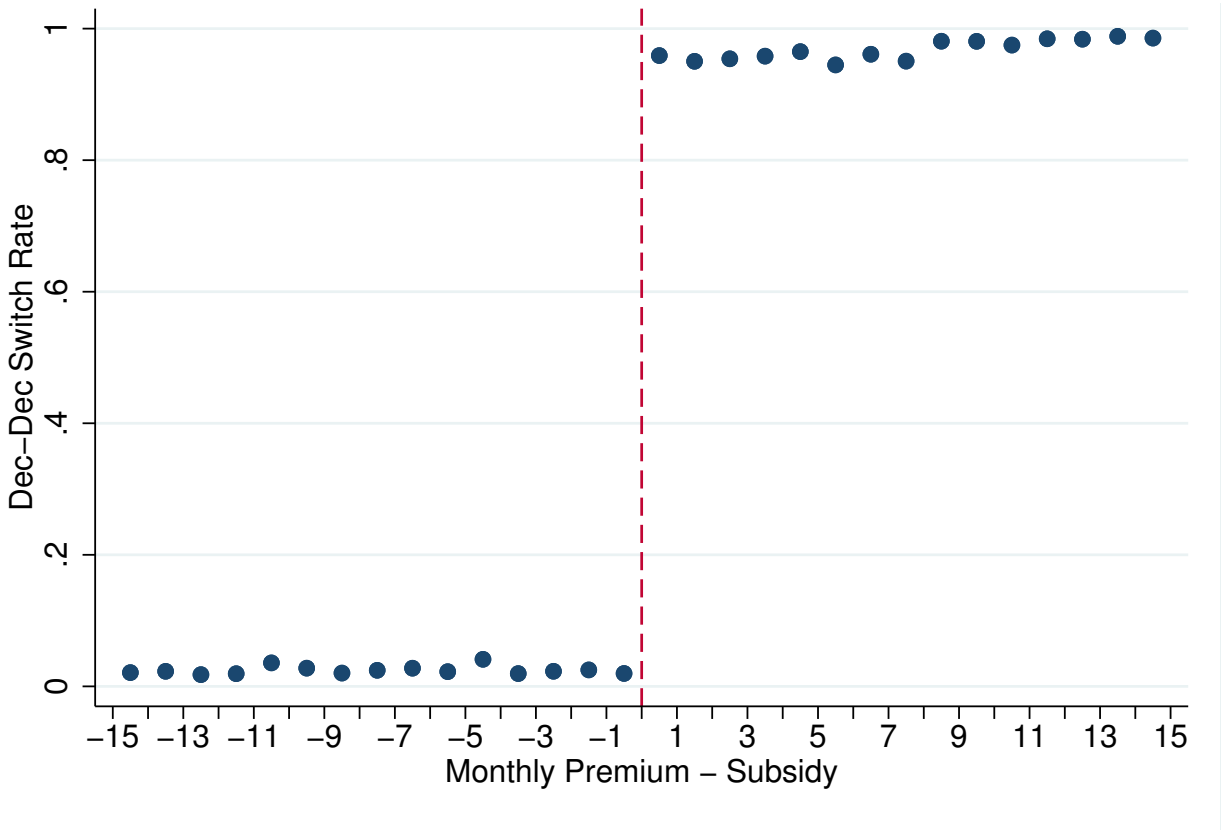
Notes: This figure plots average beneficiary characteristics in the prior year, for whole-dollar bins of the difference between their incumbent plans monthly premium bid in the current year and the regional LIS premium subsidy in the current year. We use the RD Analysis Sample described in Table 1. Panel (a) reports average beneficiary age, Panel (b) reports the share of female beneficiaries, Panel (c) reports average non-drug spending on medical services in the prior year, and panel (d) reports the average Elixhauser Comorbidity Index.



Appendix Figure A8: We break down the outcome in Figure 4 by whether the plan switch was an active choice, an active choice specifically to a non-benchmark plan, or was a reassignment through the default mechanism.



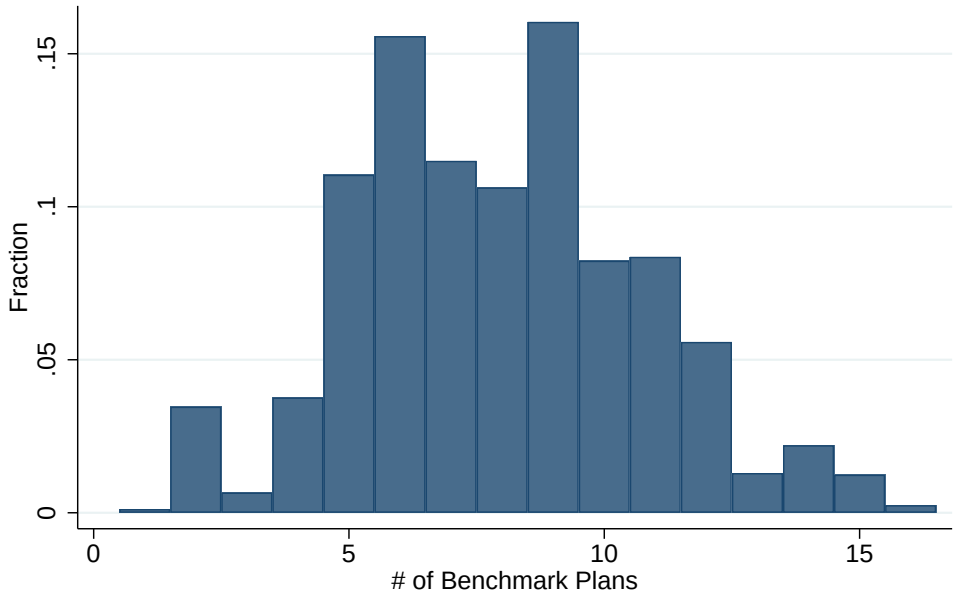
Appendix Figure A9: This figure replicates Figure 4, but additionally plots switching rates among those who had actively chosen their incumbent plan.



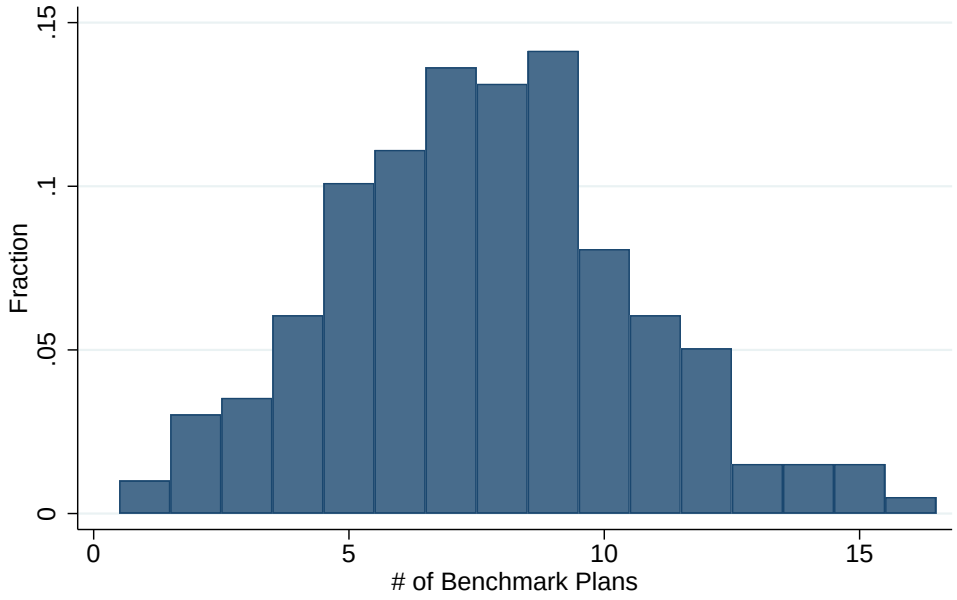
Appendix Figure A10: Probability of switching plans between December of $t-1$ and December of t, by incumbent plan premium in t.

Appendix Figure A11: Distribution of Number of Benchmark Plans in Market-Year

(a) Weighted by Number of Beneficiaries

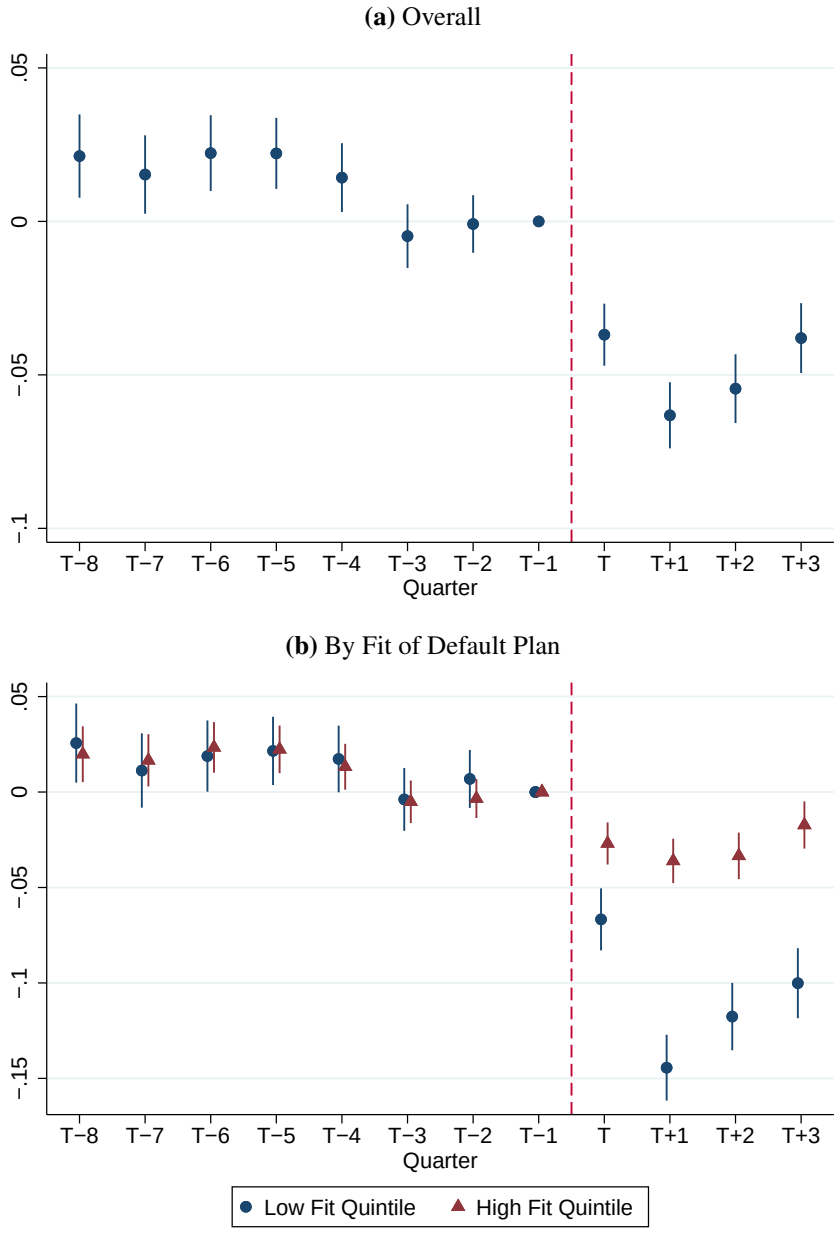


(b) Unweighted



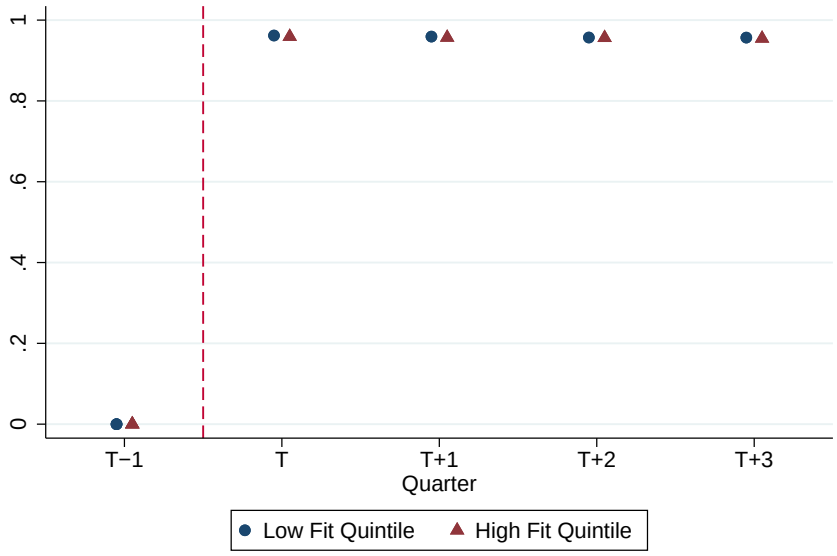
Notes: This set of figures plots the distribution in the number of benchmark plans across the combination of Part D market region-years. The top figure presents this distribution weighing all Part D market region-years equally, while the bottom weighs Part D market region-years by the number of beneficiaries in our sample enrolled under each.

Appendix Figure A12: Event Study – Effect of Change in Default from Remaining in Incumbent Plan to Auto-assignment to Randomly-Chosen Default Plan on Log Prescription Drug Spending, with Prices Normalized by Drug



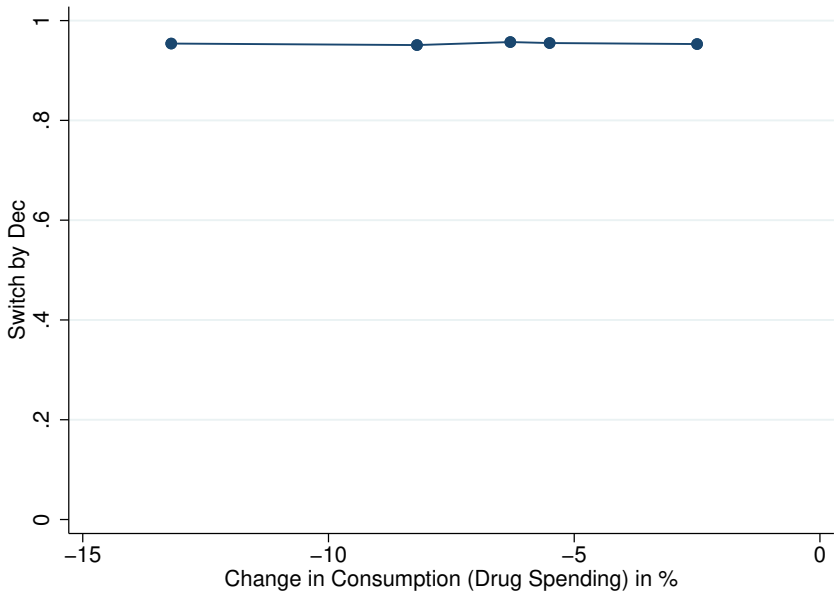
Notes: This figure plots event-study regression estimates of the effect of facing a default of reassignment to a randomly-chosen plan on quarterly drug spending outcomes, relative to facing a default of remaining in the incumbent plan. We use the 'RD Analysis Sample' described in Table 1 and estimate a stacked difference-in-differences design as described in Section 4. T represents the first quarter following potential reassignment through the default mechanism. The outcome variable is quarterly logged normalized allowed prescription drug spending. We normalize the prices for all drugs by the average price for their NDC across all plans and years before computing spending. In the third column, we normalize prices by the average price of all drugs in the same therapeutic class across all plans and years. In Panel (a), we estimate a single effect for all beneficiaries. For Panel (b), we allow for separate effects based on whether beneficiaries who faced a default of reassignment would have been assigned to a plan in the bottom quintile of plans they could have been assigned to, by the fit of that plans formulary for their own drug consumption. Red triangles represent treated beneficiaries assigned to a bottom-quintile plan, whereas blue triangles represent all other treated beneficiaries. Lines represent 95% confidence intervals.

Appendix Figure A13: Event Study Estimates of the Effect of Change in Default from Remaining in Incumbent Plan to Auto-Assignment to Randomly-Chosen Default Plan on Plan Switching Propensity, by Fit of New Default Plan



Notes: This figure plots event-study regression estimates of the effect of facing a default of reassignment to a randomly-chosen plan on plan switching, relative to facing a default of remaining in the incumbent plan. We use the RD Analysis Sample described in Table 1, and the model estimated is described in Section 6. The outcome is an indicator for whether the beneficiary was enrolled in a different plan at the end of the quarter than they were enrolled in during December of the prior year. We allow for separate effects based on whether beneficiaries who faced a default of reassignment would have been assigned to a plan in the bottom quintile of plans they could have been assigned to, by the fit of that plans formulary for their own drug consumption. Red triangles represent treated beneficiaries assigned to a bottom-quintile plan, whereas blue triangles represent all other treated beneficiaries. Lines represent 95% confidence intervals.

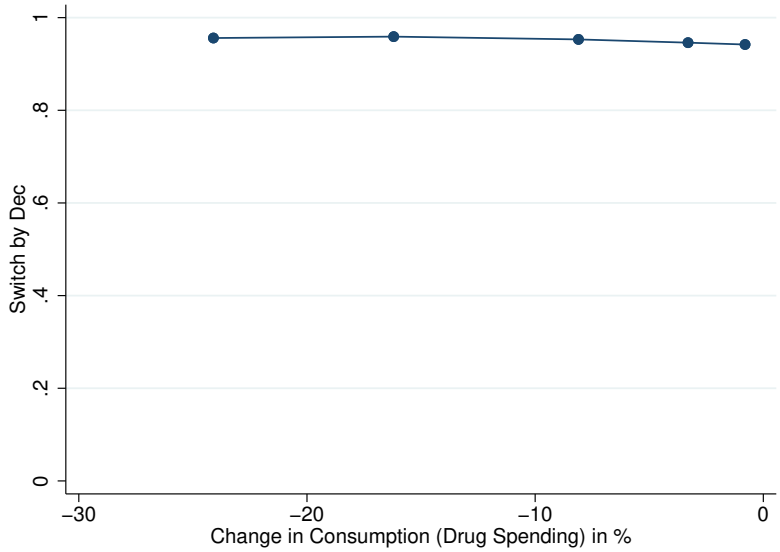
Appendix Figure A14: Comparison of Default Effects on Drug Consumption Against Default Effects on Plan Switching Propensity



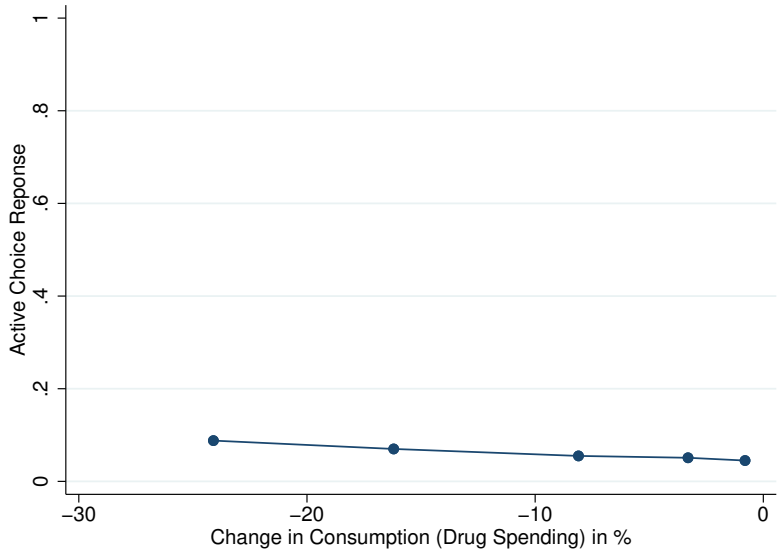
Notes: This figure plots the relationship between the effect of default plan assignment on drug spending and the effect of default plan assignment on propensity to switch plans between December of the prior year and December of the current year. To construct this figure, we estimate two difference-in-differences regressions of log drug spending and plan switching propensity on whether the beneficiary faced a default of auto-assignment to a randomly-chosen plan, interacted with the 'fit' of that plans formulary for the beneficiary's drug consumption, binned into quintiles. Each dot represents a pair of quintile-specific spending (measured in percent changed) and plan switching effects. The regressions underlying this figure are described in Sections 4 and 6, and the specific estimates are given in Appendix Table A5.

Appendix Figure A15: Rational Inattention, Top Half in Spread of Fit

(a) Plan Switch



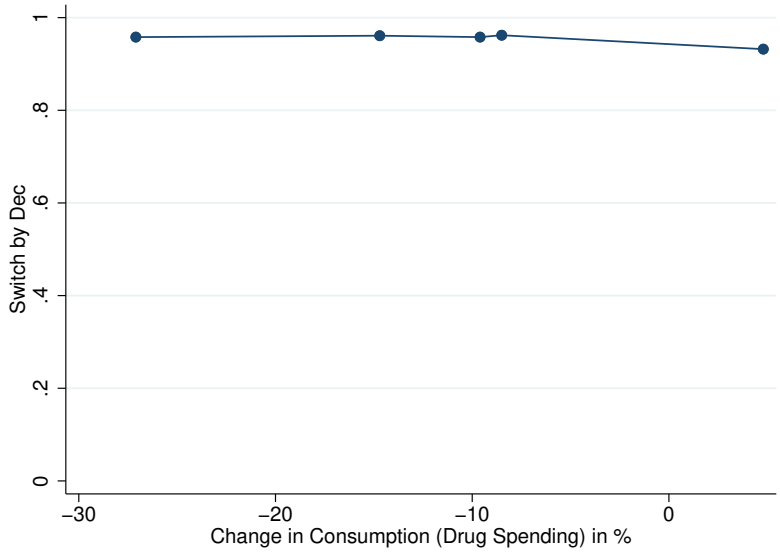
(b) Active Choice



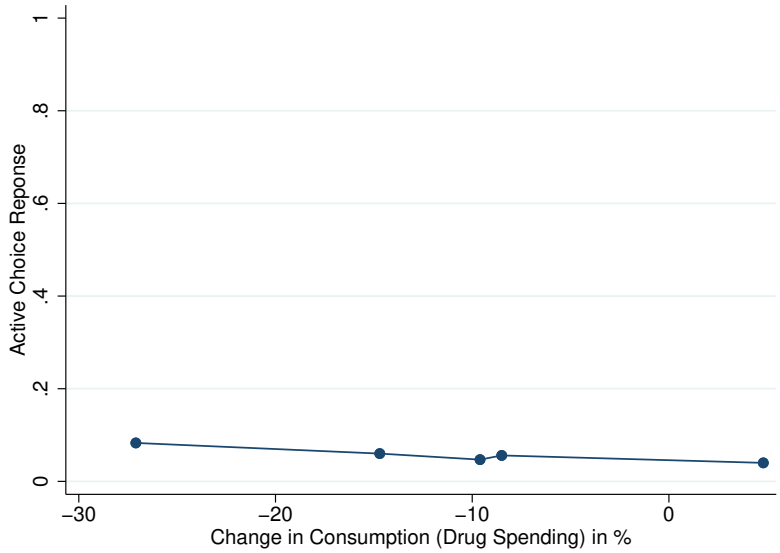
Notes: This figure replicates Figure A14 and 7, restricting to beneficiaries for whom the gap between the fit of their best plan and their worst plan is above the median gap in the sample. The top panel plots the propensity of beneficiaries to switch from the plan they were enrolled in during December of year $t-1$, as a function of the potential consumption loss they face by remaining in their default plan assignment over year t, in terms of log drug spending. The bottom panel plots the propensity of beneficiaries to make an active choice in December of year t, as a function of the potential consumption loss they face by remaining in their default plan assignment. In both panels, estimates of consumption loss come from a regression of log drug spending on whether a beneficiary's plan loses benchmark status in year t, and includes beneficiary-experiment level fixed effects, Part D region-experiment year fixed effects, and experiment pre/post year indicators. Estimates of propensity to switch and propensity to make an active choice come from regressions also including whether a beneficiary's plan lost benchmark status in year t, as well as region-experiment year fixed effects. Each regression is run for five different treatment groups, based on quintile of assigned plan fit, while full control group in all regressions. Matching coefficients from each of these five regression specifications are then plotted in the figures.

Appendix Figure A16: Rational Inattention, Top Quarter in Spread of Fit

(a) Plan Switch

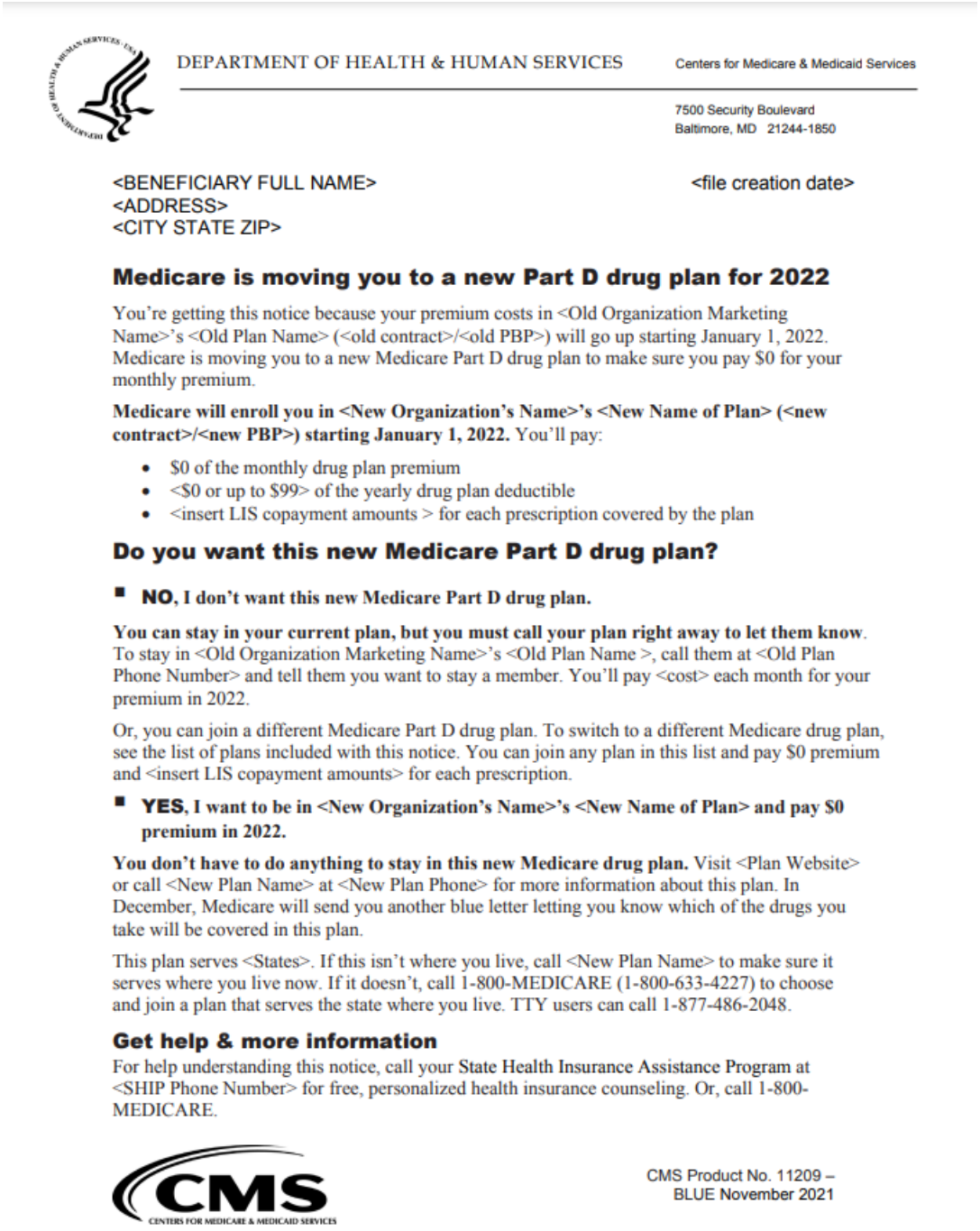


(b) Active Choice



Notes: This figure replicates Figure A14 and 7, restricting to beneficiaries for whom the gap between the fit of their best plan and their worst plan is above the 75th percentile gap in the sample. The top panel plots the propensity of beneficiaries to switch from the plan they were enrolled in during December of year $t-1$, as a function of the potential consumption loss they face by remaining in their default plan assignment over year t, in terms of log drug spending. The bottom panel plots the propensity of beneficiaries to make an active choice in December of year t, as a function of the potential consumption loss they face by remaining in their default plan assignment. In both panels, estimates of consumption loss come from a regression of log drug spending on whether a beneficiary's plan loses benchmark status in year t, and includes beneficiary-experiment level fixed effects, Part D region-experiment year fixed effects, and experiment pre/post year indicators. Estimates of propensity to switch and propensity to make an active choice come from regressions also including whether a beneficiary's plan lost benchmark status in year t, as well as region-experiment year fixed effects. Each regression is run for five different treatment groups, based on quintile of assigned plan fit, while full control group in all regressions. Matching coefficients from each of these five regression specifications are then plotted in the figures.

Appendix Figure A17: Letter Sent to Beneficiaries Whose Plan Lost Benchmark Status



DEPARTMENT OF HEALTH & HUMAN SERVICES

Centers for Medicare & Medicaid Services

7500 Security Boulevard
Baltimore, MD 21244-1850

<BENEFICIARY FULL NAME>
<ADDRESS>
<CITY STATE ZIP>

<file creation date>

Medicare is moving you to a new Part D drug plan for 2022

You're getting this notice because your premium costs in <Old Organization Marketing Name>'s <Old Plan Name> (<old contract>/<old PBP>) will go up starting January 1, 2022. Medicare is moving you to a new Medicare Part D drug plan to make sure you pay $0 for your monthly premium.

Medicare will enroll you in <New Organization's Name>'s <New Name of Plan> (<new contract>/<new PBP>) starting January 1, 2022. You'll pay:

- $0 of the monthly drug plan premium
- <$0 or up to $99> of the yearly drug plan deductible
- <insert LIS copayment amounts > for each prescription covered by the plan

Do you want this new Medicare Part D drug plan?

■ **NO, I don't want this new Medicare Part D drug plan.**

You can stay in your current plan, but you must call your plan right away to let them know. To stay in <Old Organization Marketing Name>'s <Old Plan Name >, call them at <Old Plan Phone Number> and tell them you want to stay a member. You'll pay <cost> each month for your premium in 2022.

Or, you can join a different Medicare Part D drug plan. To switch to a different Medicare drug plan, see the list of plans included with this notice. You can join any plan in this list and pay $0 premium and <insert LIS copayment amounts> for each prescription.

■ **YES, I want to be in <New Organization's Name>'s <New Name of Plan> and pay $0 premium in 2022.**

You don't have to do anything to stay in this new Medicare drug plan. Visit <Plan Website> or call <New Plan Name> at <New Plan Phone> for more information about this plan. In December, Medicare will send you another blue letter letting you know which of the drugs you take will be covered in this plan.

This plan serves <States>. If this isn't where you live, call <New Plan Name> to make sure it serves where you live now. If it doesn't, call 1-800-MEDICARE (1-800-633-4227) to choose and join a plan that serves the state where you live. TTY users can call 1-877-486-2048.

Get help & more information

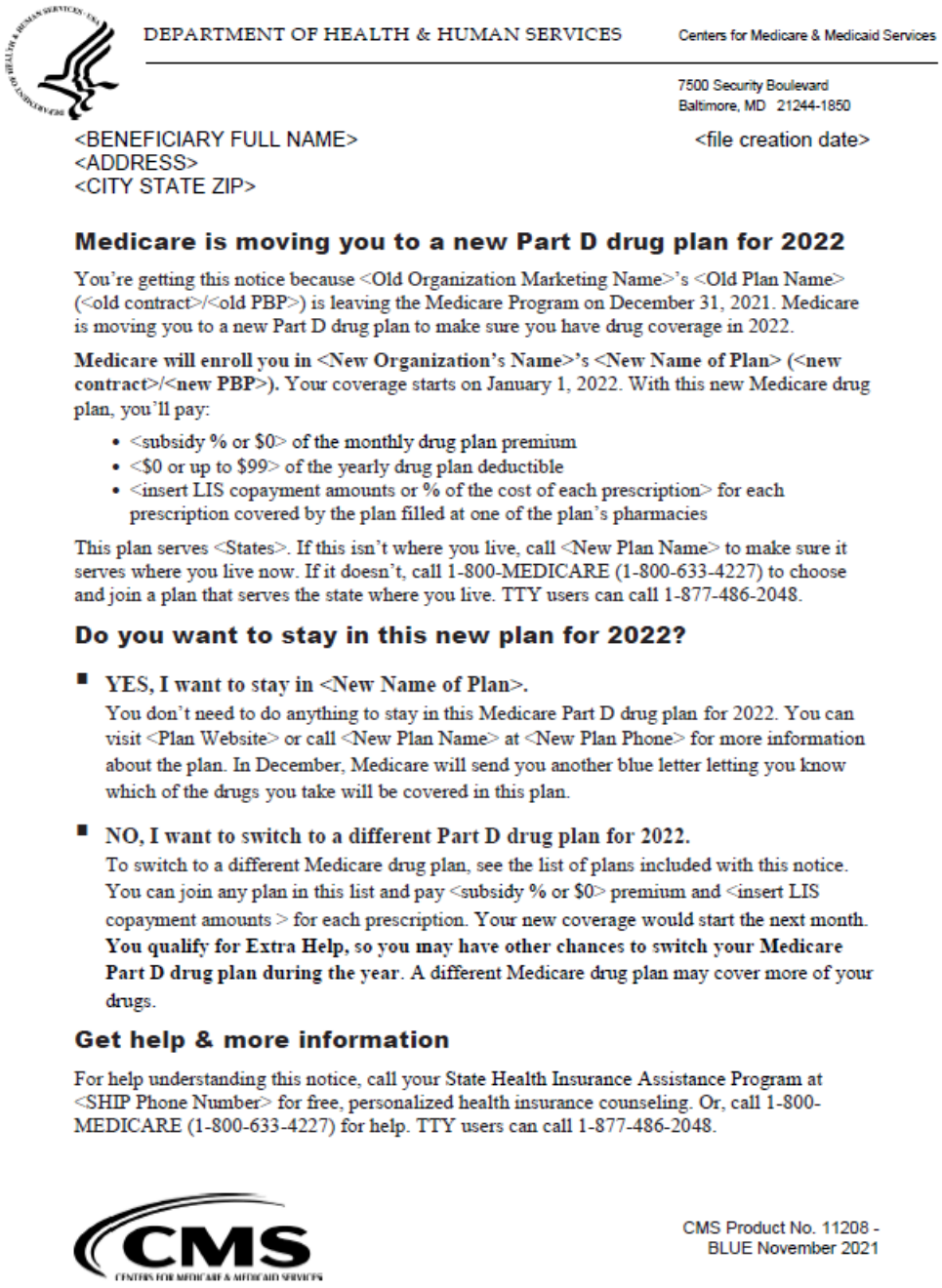
For help understanding this notice, call your State Health Insurance Assistance Program at <SHIP Phone Number> for free, personalized health insurance counseling. Or, call 1-800-MEDICARE.

CMS
CENTERS FOR MEDICARE & MEDICAID SERVICES

CMS Product No. 11209 –
BLUE November 2021

Notes: This figure displays a template for the official letter sent by CMS to LIS beneficiaries who were previously auto-enrolled in a plan which lost benchmark status in 2022. We display only the first page here. The following page gives a list of potential plan options, with their contact information.

Appendix Figure A18: Letter Sent to Beneficiaries Whose Plan Exited



DEPARTMENT OF HEALTH & HUMAN SERVICES

Centers for Medicare & Medicaid Services

7500 Security Boulevard
Baltimore, MD 21244-1850

<BENEFICIARY FULL NAME>
<ADDRESS>
<CITY STATE ZIP>

<file creation date>

Medicare is moving you to a new Part D drug plan for 2022

You're getting this notice because <Old Organization Marketing Name>'s <Old Plan Name> (<old contract>/<old PBP>) is leaving the Medicare Program on December 31, 2021. Medicare is moving you to a new Part D drug plan to make sure you have drug coverage in 2022.

Medicare will enroll you in <New Organization's Name>'s <New Name of Plan> (<new contract>/<new PBP>). Your coverage starts on January 1, 2022. With this new Medicare drug plan, you'll pay:

- <subsidy % or $0> of the monthly drug plan premium
- <$0 or up to $99> of the yearly drug plan deductible
- <insert LIS copayment amounts or % of the cost of each prescription> for each prescription covered by the plan filled at one of the plan's pharmacies

This plan serves <States>. If this isn't where you live, call <New Plan Name> to make sure it serves where you live now. If it doesn't, call 1-800-MEDICARE (1-800-633-4227) to choose and join a plan that serves the state where you live. TTY users can call 1-877-486-2048.

Do you want to stay in this new plan for 2022?

- **YES, I want to stay in <New Name of Plan>.**
  You don't need to do anything to stay in this Medicare Part D drug plan for 2022. You can visit <Plan Website> or call <New Plan Name> at <New Plan Phone> for more information about the plan. In December, Medicare will send you another blue letter letting you know which of the drugs you take will be covered in this plan.

- **NO, I want to switch to a different Part D drug plan for 2022.**
  To switch to a different Medicare drug plan, see the list of plans included with this notice. You can join any plan in this list and pay <subsidy % or $0> premium and <insert LIS copayment amounts > for each prescription. Your new coverage would start the next month. **You qualify for Extra Help, so you may have other chances to switch your Medicare Part D drug plan during the year.** A different Medicare drug plan may cover more of your drugs.

Get help & more information

For help understanding this notice, call your State Health Insurance Assistance Program at <SHIP Phone Number> for free, personalized health insurance counseling. Or, call 1-800-MEDICARE (1-800-633-4227) for help. TTY users can call 1-877-486-2048.

CMS

CENTERS FOR MEDICARE & MEDICAID SERVICES

CMS Product No. 11208 -
BLUE November 2021

Notes: This figure displays a template for the official letter sent by CMS to LIS beneficiaries who were previously auto-enrolled in a plan which exited in 2022. We display only the first page here. The following page gives a list of potential plan options, with their contact information.

Appendix Table A1: Regression Discontinuity Balance Tests for Observable Beneficiary Characteristics

	Age	% Female	Prior Drug Spending	Prior Non-Drug Med. Spending	Elixhauser Index	Incumbent Plan 'Fit'
Beneficiary Faced Plan Reassignment Default	0.021 (0.034)	0 (0.002)	-23.484 (19.283)	-23.102 (85.557)	-0.001 (0.013)	-0.015 (0.001)
Average Value for Control Beneficiaries	76.3	0.682	3,328	9,528	3.88	0.632
Observations			464,341			

Notes: This table reports regression discontinuity balance tests for observable beneficiary characteristics, illustrated by coefficients on a dummy variable for the beneficiary's year $t-1$ plan having a year t bid that exceeds the year t benchmark. For this, the RD analytic sample is employed, which is described in further detail in Table 1. RD regressions include Part D region-experiment year fixed effects. All regressions restrict to beneficiaries whose year $t-1$ plans bid within \$6 of the year t benchmark. Regression coefficients summarize the results in Figure 3.

Appendix Table A2: Difference-in-Difference Regression Estimates of the Effects of a Change in Default from Remaining in Incumbent Plan to Auto-assignment to Randomly-Selected Default Plan on Drug and Non-Drug Outcomes

	Log # Fills	Log Days Supply	Log Non-Drug Spending
Panel A			
Faced Plan Reassignment Default	-0.005	-0.015	0.006
× Post	(0.002)	(0.003)	(0.006)
Panel B			
Faced Plan Reassignment Default	-0.002	-0.007	0.010
× Post	(0.002)	(0.004)	(0.007)
Faced Plan Reassignment Default	-0.011	-0.030	-0.017
× Assigned Plan in Worst Quintile by Fit	(0.003)	(0.006)	(0.010)
× Post			
Average Value for Control Beneficiaries in Prior Year (levels)	53.3	1649	$9535
Observations		5,574,684	

Notes: This table reports difference-in-difference regression estimates of the change in a beneficiary's default from remaining in their incumbent plan to auto-assignment to a randomly-selected default plan on quarterly drug utilization and non-drug spend outcomes. The treatment group consists of beneficiaries whose year $t-1$ plans bid above the year t benchmark, and the control group consists of beneficiaries whose year $t-1$ plans bid below the year t benchmark. For this, the RD analytic sample is employed, which is described in further detail elsewhere. For both groups, we restrict to beneficiaries whose year $t-1$ plans bid within $6 of the year t benchmark. Regressions include beneficiary-experiment level fixed effects, Part D region-experiment year fixed effects, and experiment-pre/post year indicators. In Panel A, we estimate the overall effect of the change in default. In Panel B, we estimate heterogeneous effects by the fit of the plan (randomly) selected to be the beneficiary's year t default by interacting an indicator for being assigned a default plan in the bottom quintile of fit among the benchmark plans in the beneficiary's choice set with dummies for "Post" and $1(Bid_t > Benchmark_t)$. As such, the coefficients in the first row of Panel B can be interpreted as the effects of the change in default for beneficiaries whose randomly-selected default plan is in the top four quintiles of fit, and the sum of the coefficients in the first and second rows of Panel B can be interpreted as the effects of the change in default for beneficiaries whose randomly-selected default plan is in the bottom quintile of fit. In column (1), the outcome is number of prescription fills, while the outcome in column (2) is total days supply associated with those fills. In column (3) meanwhile, the outcome is non-drug medical spending, defined as spending under Medicare Parts A and B.

Appendix Table A3: Event Study - Effect of Change in Default from Remaining in Incumbent Plan to Auto- assignment to Randomly-Chosen Default Plan

	Switch from t - 1 Plan by December of t	Active Choice by December of t	Log (Drug Spending)	Log (Price-Normalized Drug Spending)
T - 8			0.013	0.021
			(0.007)	(0.007)
T - 7			0.001	0.015
			(0.007)	(0.007)
T - 6			0.009	0.022
			(0.006)	(0.006)
T - 5			0.009	0.022
			(0.006)	(0.006)
T - 4			0.011	0.014
			(0.006)	(0.006)
T - 3			-0.003	-0.005
			(0.005)	(0.005)
T - 2			0.004	-0.001
			(0.005)	(0.005)
T	0.960	0.046	-0.041	-0.037
	(0.001)	(0.001)	(0.005)	(0.005)
T + 1	0.957	0.049	-0.077	-0.063
	(0.001)	(0.001)	(0.006)	(0.005)
T + 2	0.956	0.050	-0.075	-0.054
	(0.001)	(0.001)	(0.006)	(0.006)
T + 3	0.955	0.050	-0.050	-0.038
	(0.001)	(0.001)	(0.006)	(0.006)
Mean of Dep Var	0.170	0.016	5.390	5.451
Observations	2,307,468	2,307,441	5,574,672	5,574,672

Notes: Regression results underlying Figure 5

Appendix Table A4: Event Study - Effect of Change in Default from Remaining in Incumbent Plan to Auto- assignment to Randomly-Chosen Default Plan

	Switch from t - 1 Plan by December of t	Active Choice by December of t	Log (Drug Spending)	Log (Price-Normalized Drug Spending)
T - 8 (low fit)			0.018	0.026
			(0.011)	(0.011)
T - 8 (high fit)			0.011	0.020
			(0.007)	(0.007)
T - 7 (low fit)			-0.003	0.011
			(0.010)	(0.010)
T - 7 (high fit)			0.002	0.017
			(0.007)	(0.007)
T - 6 (low fit)			0.006	0.019
			(0.010)	(0.010)
T - 6 (high fit)			0.009	0.023
			(0.007)	(0.007)
T - 5 (low fit)			0.011	0.022
			(0.009)	(0.009)
T - 5 (high fit)			0.009	0.022
			(0.006)	(0.006)
T - 4 (low fit)			0.015	0.017
			(0.009)	(0.009)
T - 4 (high fit)			0.009	0.013
			(0.006)	(0.006)
T - 3 (low fit)			-0.002	-0.004
			(0.008)	(0.008)
T - 3 (high fit)			-0.004	-0.005
			(0.006)	(0.006)
T - 2 (low fit)			0.012	0.007
			(0.008)	(0.008)
T - 2 (high fit)			0.001	-0.003
			(0.005)	(0.005)
T (low fit)	0.962	0.055	-0.071	-0.067
	(0.001)	(0.002)	(0.008)	(0.008)
T (high fit)	0.959	0.043	-0.031	-0.027
	(0.001)	(0.001)	(0.006)	(0.006)
T + 1 (low fit)	0.959	0.059	-0.159	-0.144
	(0.001)	(0.002)	(0.009)	(0.009)
T + 1 (high fit)	0.957	0.046	-0.050	-0.036
	(0.001)	(0.001)	(0.006)	(0.006)
T + 2 (low fit)	0.957	0.061	-0.142	-0.118
	(0.001)	(0.002)	(0.009)	(0.009)
T + 2 (high fit)	0.956	0.047	-0.053	-0.033
	(0.001)	(0.001)	(0.006)	(0.006)
T + 3 (low fit)	0.957	0.062	-0.109	-0.100
	(0.001)	(0.002)	(0.009)	(0.009)
T + 3 (high fit)	0.955	0.047	-0.030	-0.017
	(0.001)	(0.001)	(0.006)	(0.006)
Mean of Dep Var	0.170	0.016	5.390	5.451
Observations	2,307,468	2,307,441	5,574,672	5,574,672

Notes: Regression results underlying Figures 5b, A12b, A14, 6.

Appendix Table A5: Comparison of the Reduction in Consumption Due to the Change in Default Versus Effects of the Change in Default in Switching and Active Choice

	Fit Quintile					Difference Between 1 & 5	
	1	2	3	4	5	Effect	$
Primary Sample							
Active Choice Response	0.073	0.063	0.054	0.051	0.050	0.023	
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		
Utilization Response	-0.132	-0.082	-0.063	-0.055	-0.025	-0.107	$396
	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)		
Top 50% Spread Sample							
Active Choice Response	0.088	0.070	0.055	0.051	0.045	0.043	
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)		
Utilization Response	-0.241	-0.162	-0.081	-0.033	-0.008	-0.233	$863
	(0.009)	(0.010)	(0.010)	(0.010)	(0.011)		
Top 25% Spread Sample							
Active Choice Response	0.083	0.060	0.056	0.047	0.040	0.043	
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)		
Utilization Response	-0.271	-0.147	-0.085	-0.096	0.048	-0.271†	$1,004
	(0.014)	(0.015)	(0.015)	(0.016)	(0.015)		
Top 10% Spread Sample							
Active Choice Response	0.076	0.053	0.043	0.044	0.034	0.042	
	(0.006)	(0.006)	(0.005)	(0.006)	(0.004)		
Utilization Response	-0.300	-0.131	-0.085	-0.140	0.011	-0.300†	$1,112
	(0.023)	(0.028)	(0.026)	(0.028)	(0.026)		

Notes: This table reports results from an alternative specification to that of Equation 3 where we interact the indicator for facing default reassignment with an indicator for every fit quintile separately rather than comparing the lowest quintile (1) to the other four. Effects are measured relative to control beneficiaries whose default was to remain in their incumbent plan, and are reported in the first five columns. The sixth column displays the difference in treatment effects between the bottom (1st) and top (5th) quintiles; e.g., under our original sample, beneficiaries assigned to plans in the bottom quintile of fit are 2.3pp more likely to make an active choice than beneficiaries assigned to plans in the top quintile of fit. The seventh column converts utilization responses into equivalent dollar effects. Each panel reflects regression results in a different subsample; the top panel reflects our primary sample, while the others reflect subsamples limiting to beneficiaries with the most variation in fit across benchmark plans.

† In these specifications, we treat the effect of being assigned to a plan in the top quintile of fit as 0 rather than the estimated effect.

Appendix Table A6: Effects of Plan Assignment on Spending for Beneficiaries With Large Variance in Fit Across Plans, Price Normalization

		Subsample (Fit Variance Quantile)		
	All	Top 50th	Top 25th	Top 10th
	Outcome: Spending with Drug-Level Price Normalization			
Faced Plan Reassignment Default	-0.030	-0.054	-0.046	-0.026
× Post	(0.004)	(0.010)	(0.015)	(0.025)
Faced Plan Reassignment Default	-0.082	-0.196	-0.218	-0.216
× Assigned Plan in Worst Quintile by Fit	(0.007)	(0.010)	(0.015)	(0.026)
× Post				
	Outcome: Spending with Class-Level Price Normalization			
Faced Plan Reassignment Default	-0.012	-0.049	-0.042	-0.014
× Post	(0.004)	(0.009)	(0.014)	(0.024)
Faced Plan Reassignment Default	-0.036	-0.111	-0.110	-0.085
× Assigned Plan in Worst Quintile by Fit	(0.006)	(0.009)	(0.014)	(0.025)
× Post				

Notes: This table reports results from regressions in which we replicate the specifications from the second and third columns of Table 3, Panel B, restricted to subsamples. We define these subsamples based on the variance of 'fit' across benchmark plans in that beneficiary's region and year. The first column directly replicates the results from the prior tables. In the second through fourth columns, we restrict to the top 50%, 25%, and 10% of beneficiaries, ranked by this variance measure, respectively.

Appendix Table A7: Estimates of Effects of Prior Active Choice and Other Observed and Unobserved Individual Characteristics on Current Active Choice

	Made Active Choice Following Plan Exit			
	(1)	(2)	(3)	(4)
Actively Enrolled in Exiting Plan	0.179	0.175	0.119	0.237
	(0.005)	(0.005)	(0.006)	(0.011)
Years Enrolled in Exiting Plan				0.011
				(0.003)
Actively Enrolled × Years Enrolled				−0.038
				(0.002)
Demographic and Health Controls		X	X	X
Year Fixed Effects		X	X	X
Exiting Plan Fixed Effects			X	X
R^2	0.058	0.092	0.181	0.190
Share Active Choice	0.128			
Share Active Choice For Previously Non-Actively Enrolled	0.077			
Share Active Choice For Previously Actively Enroled	0.256			
Share Previously Actively Enrolled	0.283			
Number of Exiting Plans	84			
Observations	32,852			

Notes: This table reports regression estimates of the effect of whether a beneficiary had enrolled in their incumbent plan by actively choosing it on their propensity to make an active choice of plan following their incumbent plan's exit from the market. In the first column we present the results from a regression with no additional controls. In column (2), we add in a battery of controls for demographics, including age, gender, and race, as well as controls for health status, i.e. the presence of comorbidities used in the Elixhauser Comorbidity Index. In column (3), we add fixed effects for the beneficiary's exiting plan. Lastly, in column (4), we interact the beneficiary's active choice indicator with a measure of the years enrolled in the exiting plan, i.e., the years since their last active choice. The average prior chooser had been enrolled for 2.9 years in their exiting plan, while the average prior non-chooser had been enrolled for 2.4.

B Appendix: Measurement of Plan 'Fit'

In this appendix, we describe our measure of how well a plan's formulary "fits" the basket of drugs demanded by a beneficiary. For each beneficiary, we identify the basket of drugs taken by the beneficiary in year $t-1$. For new beneficiaries, $t-1$ is the year prior to the year the beneficiary entered Medicare, and the basket of drugs is constructed using data from the Medicaid programs in New York and Texas in the year prior to entering Medicare at age 65. For beneficiaries experiencing a change in their default from staying in their incumbent plan to auto-assignment to a randomly-selected default plan, $t-1$ is the year prior to the change in the default and the basket of drugs is constructed using data from that year (when beneficiaries were in their prior plan). The measure ranges from 0 (no drugs in the beneficiary's basket covered by the plan) to 1 (all drugs in the beneficiary's basket covered by the plan).

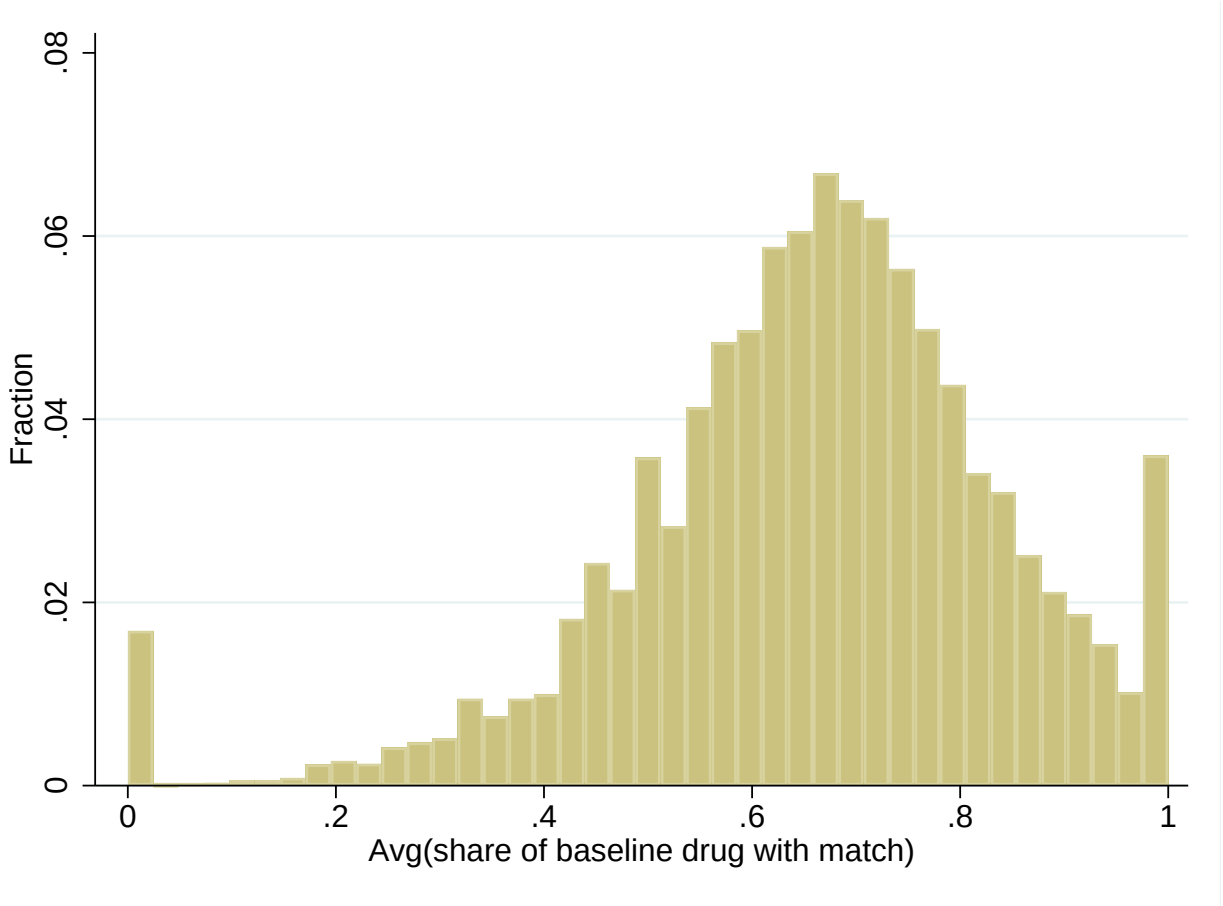
We plot the distribution of these beneficiary-plan fit measures for benchmark plans in Figure A19. Plan-beneficiary fit varies significantly for beneficiaries in the LIS program – while some plans cover 100% of a beneficiary's drugs, others cover none at all, with nearly full support across the distribution between 0 and 1. The average benchmark plan covers 60% of a beneficiary's prescriptions with a standard deviation of 23 percentage points.

Ultimately, we use the measure of plan-beneficiary fit to (1) measure the value of active choice and (2) assess the consequences of being assigned to a low-fitting plan versus a high-fitting plan. Both of these uses depend more on a beneficiary facing *differences* in fit across plans rather than lower or higher average fit across plans. Because of this, we present information on the *within-beneficiary* variation in fit across plans in the beneficiary's choice set. Figure A20 plots, for each beneficiary, the difference between the fit of the best fitting plan and the average fit across plans in the beneficiary's choice set.[33] This distribution is more compressed than the distribution of fit, owing to the correlation between fit values across plans for a given beneficiary. Despite this, we still see a great deal of heterogeneity, with a significant portion of beneficiaries facing relative coverage gaps of over 10 percentage points and some beneficiaries facing relative coverage gaps nearing half of their drugs.

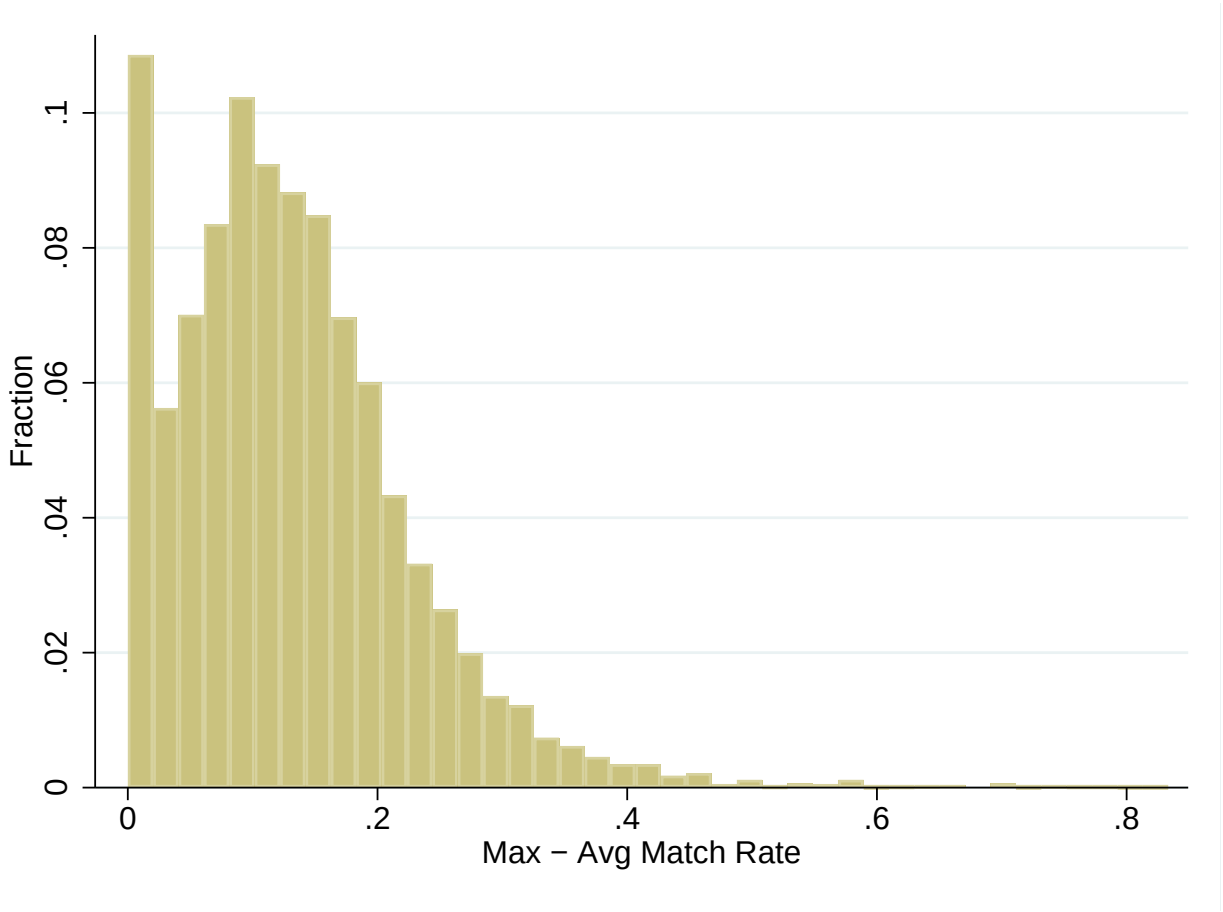
In Figure A21, we create a plot similar to Figure 1b, but using quartiles of the raw difference between the best-fitting plan a beneficiary has access to and the average, another representation of the "value of choice." In Section 6 we explain why using absolute differences may not be ideal. However, it produces results similar to that of Figure 1b.

Additionally, our measure of fit weights each drug in the beneficiary's basket as equivalent. In fact, some drugs may be of much higher value to the beneficiary than others. Naturally, we have no way of assessing beneficiary drug-specific value. As a proxy, we construct an alternative 'fit' measure that weights each drug by its average transacted price, so that our 'fit' measure is the share of *spending* covered by a plan, rather than the share of drugs. We replicate Figures A19, A20, A21, 1b, and 5b with this measure in, respectively, Appendix Figures A22 through A26.

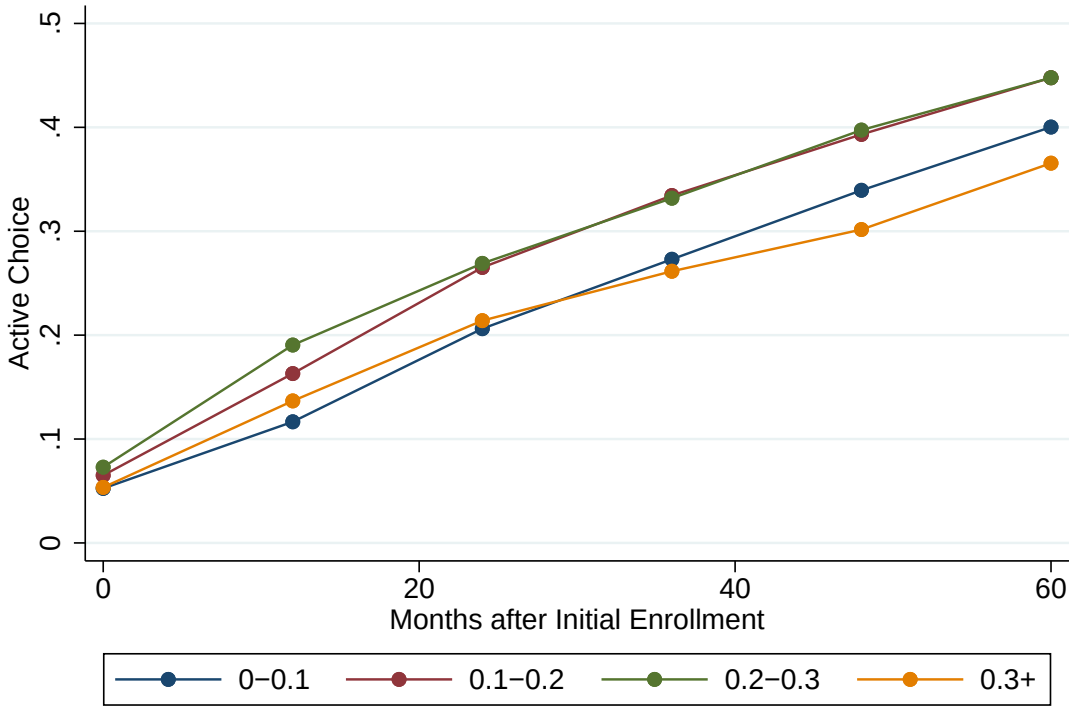
[33] One important note is that we take the maximum fit value from only the set of benchmark plans. Some beneficiaries might be able to improve their coverage by choosing a non-free insurance option. In practice, few LIS beneficiaries end up in such plans. Our approach is agnostic about the beneficiary's preferred trade-off between formulary coverage and premium payments and therefore serves as a lower bound on the true value of choice.



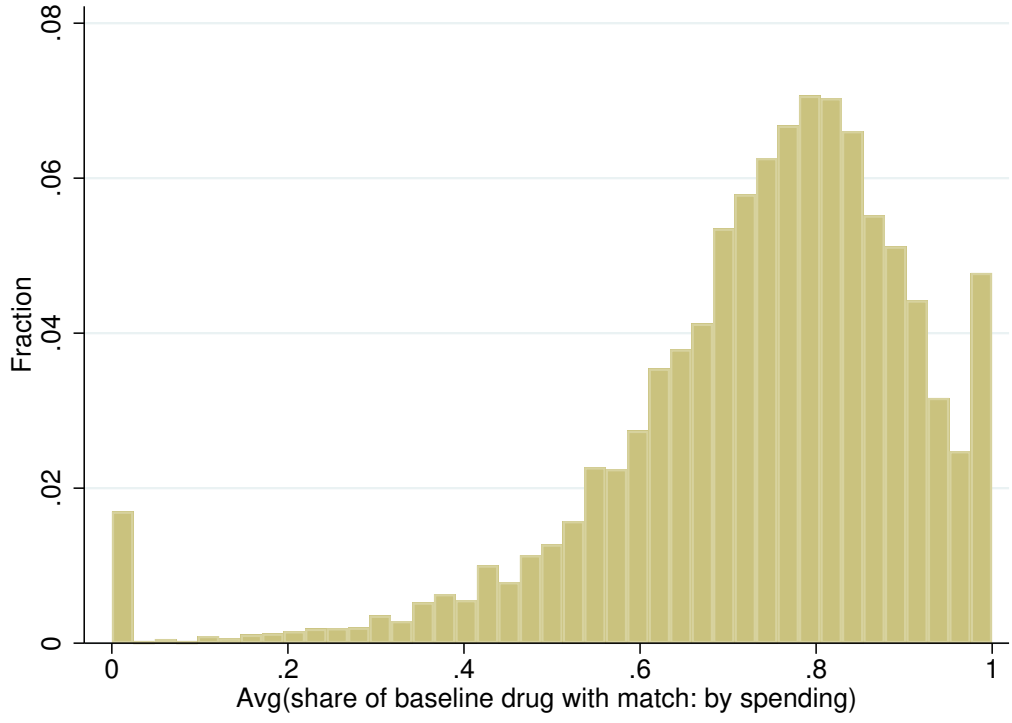
Appendix Figure A19: Distribution of beneficiary-plan pairwise fit measures for our Medicaid-linked sample of 65-year-olds, where fit is defined as the share of drugs taken at 64 covered by the plan. Fit is only computed for benchmark plans available when they turned 65.



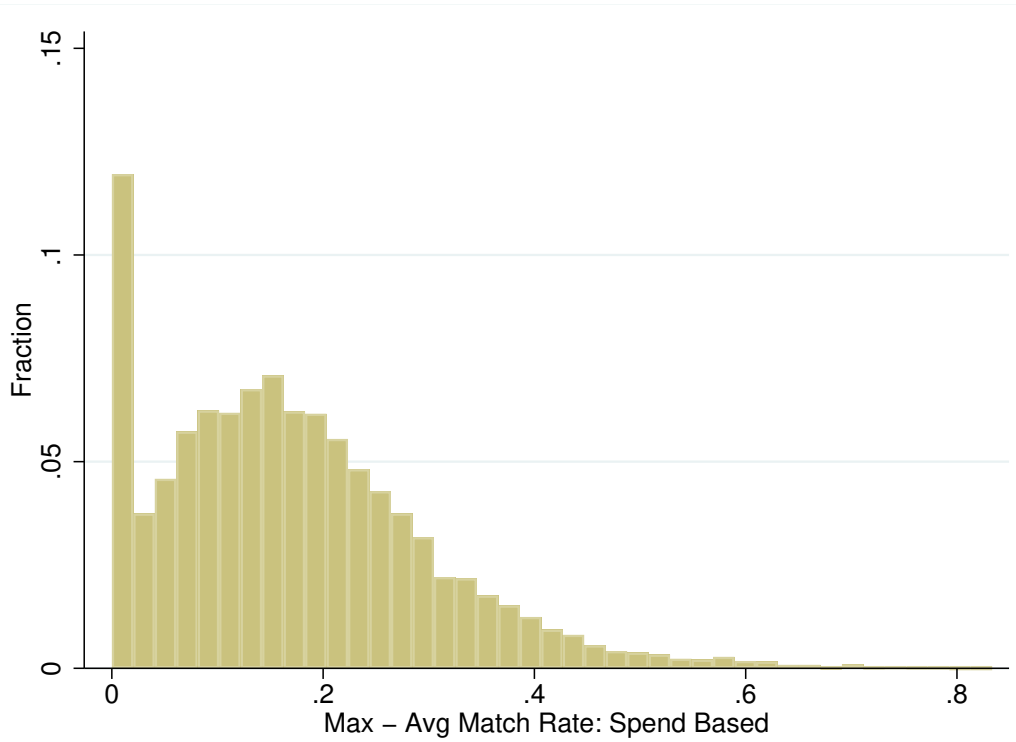
Appendix Figure A20: Distribution of beneficiary-specific measures of the difference between the fit of the best-fitting plan, and the average across available benchmark plans when they turned 65, for our Medicaid-linked sample of 65-year olds.



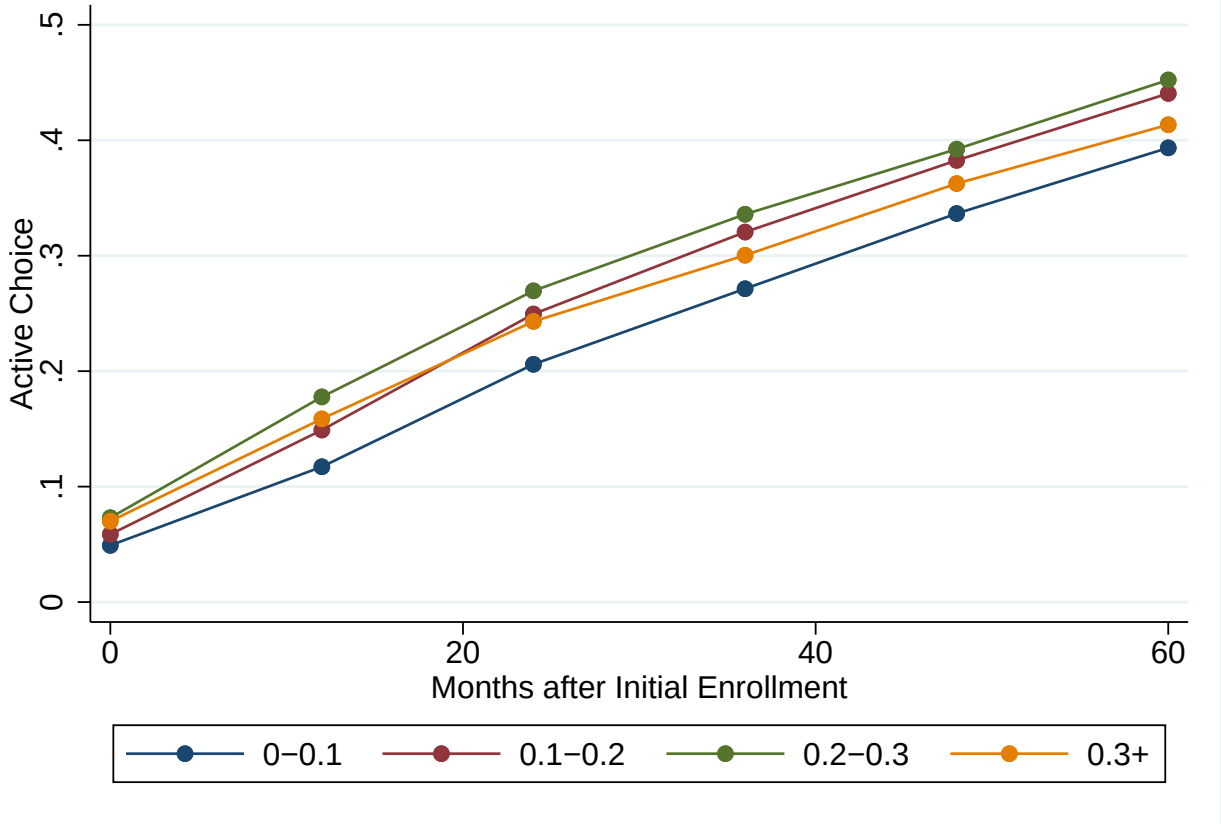
Appendix Figure A21: Cumulative active choice propensity for our Medicaid-linked sample by quartile of our 'value of choice' measure.



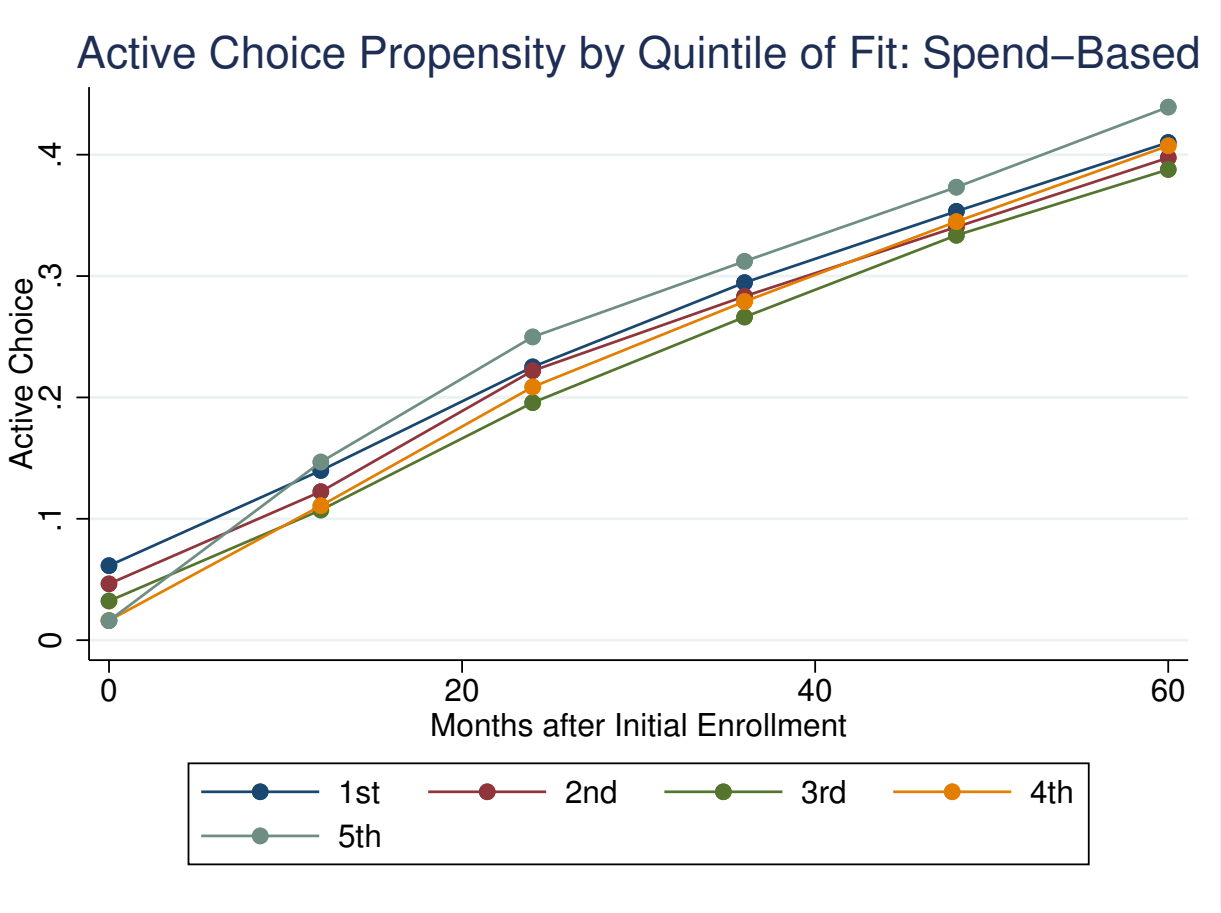
Appendix Figure A22: This figure replicates Figure A19 using our price-weighted measure of fit.



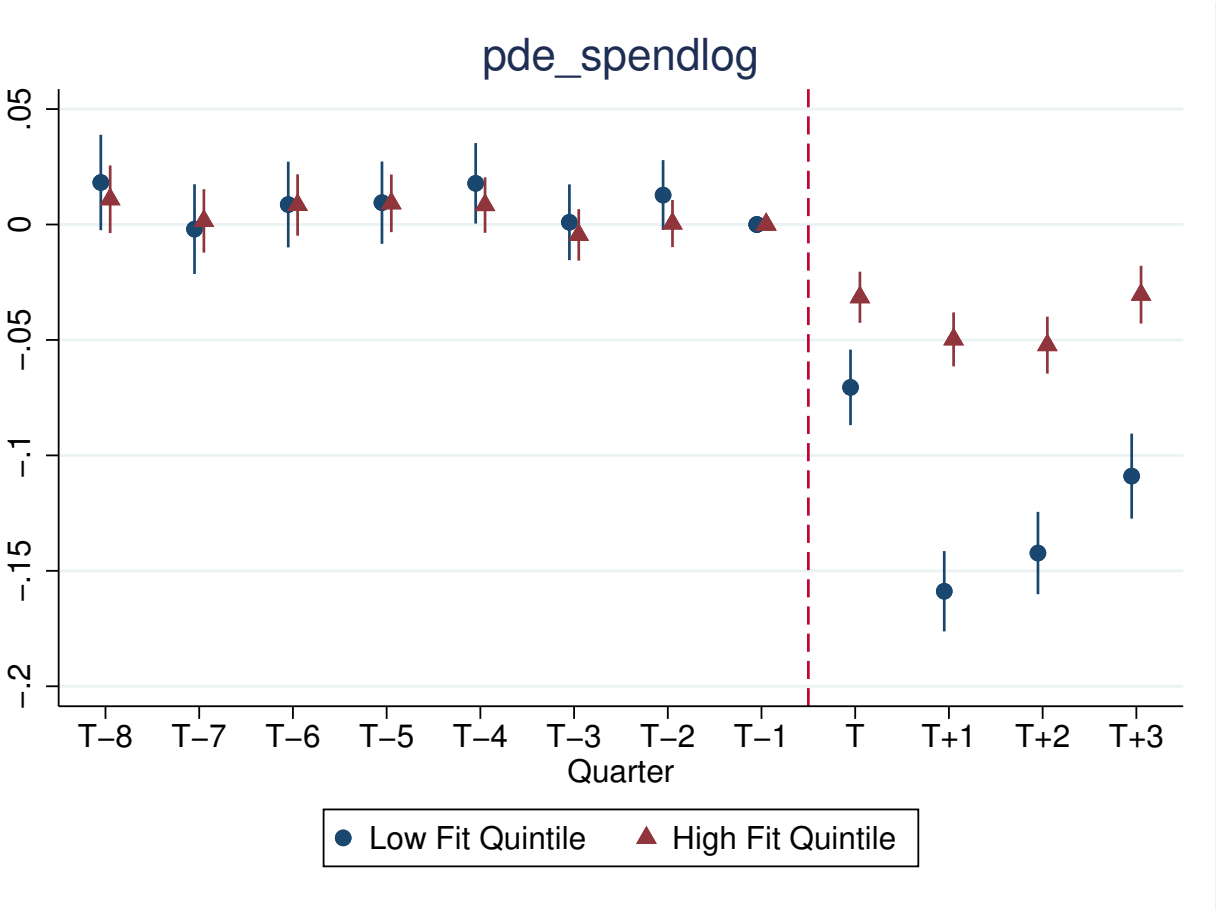
Appendix Figure A23: This figure replicates Figure A20 using our price-weighted measure of fit.



Appendix Figure A24: This figure replicates Figure A21 using our price-weighted measure of fit.



Appendix Figure A25: This figure replicates Figure 1b using our price-weighted measure of fit.



Appendix Figure A26: This figure replicates Figure 5b using our price-weighted measure of fit.

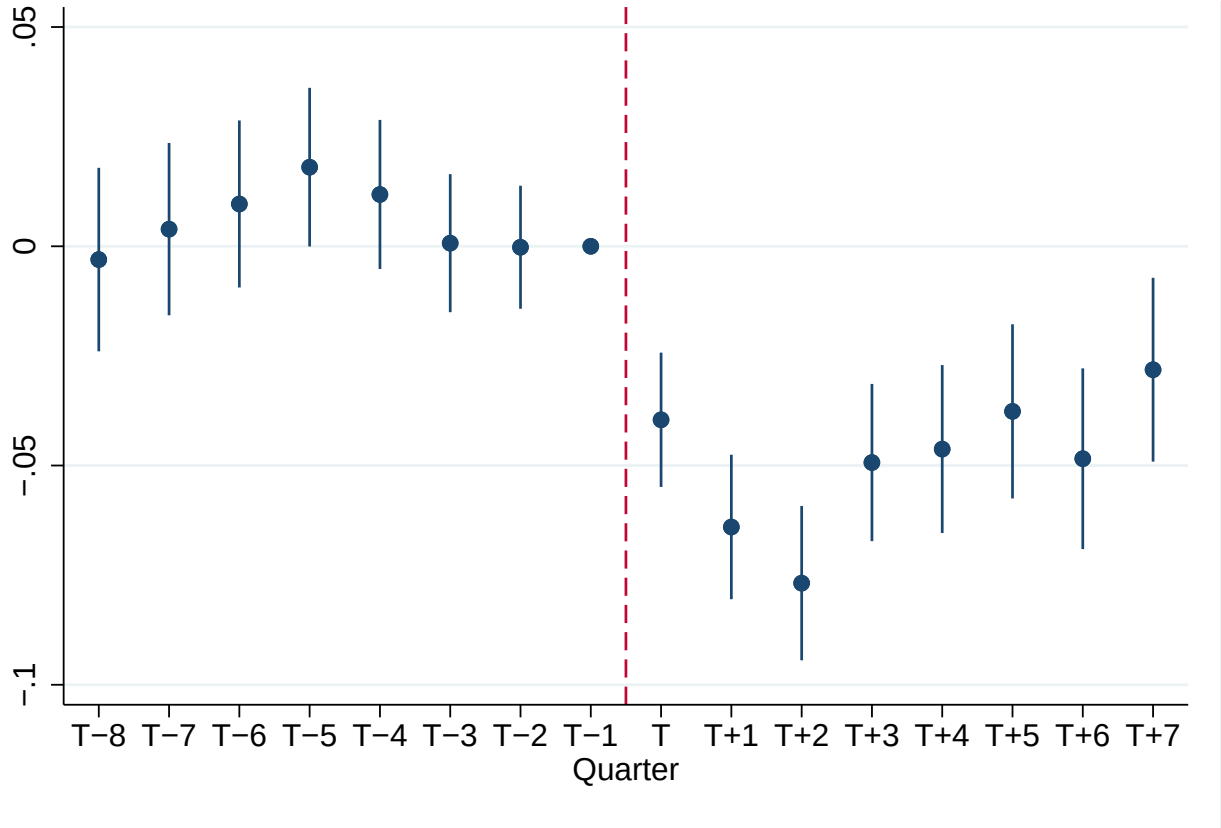
C Appendix: Long-Run Effects

In Sections 4 and 6, we use a regression discontinuity design leveraging the fact that when a $t-1$ benchmark plan submits a year t bid exceeding the year t subsidy, the beneficiaries in that plan face an exogenous change in their default from remaining in their year t plan to auto-assignment to a randomly-chosen default plan. We use that RD design to estimate the effects of the change in default on the probability of switching plans and the probability of making an active choice. For those analyses, we study switching and active choice within 12 months of the change in default. We limit to only 12 months because, following that year, many plans that were benchmark plans in year t (and thus enroll both control beneficiaries and reassigned treated beneficiaries) may themselves lose benchmark status in year $t+1$. Because of this, interpreting treatment effects in future years is tricky, as we must disentangle the treatment effect of the benchmark loss in t *and* the potential loss in $t+1$.

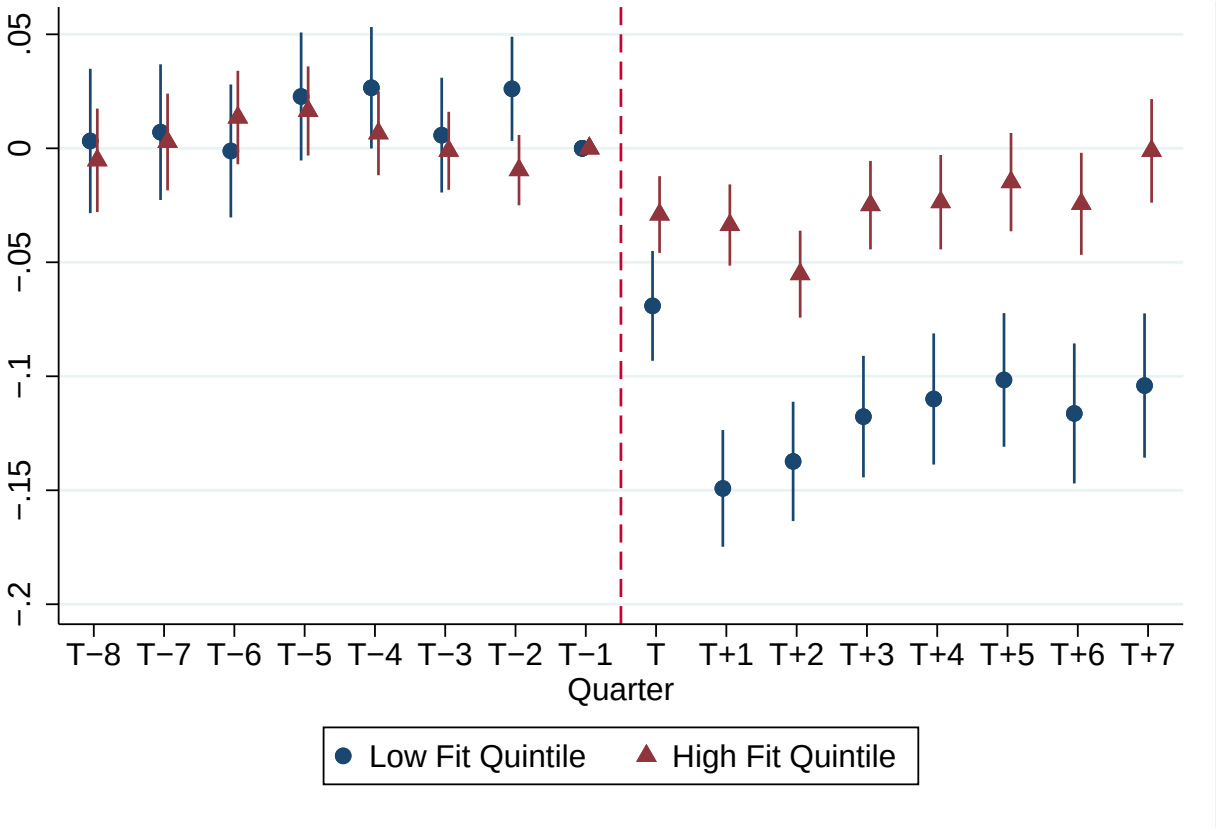
In this section, we study the effects of the change in default on switching and active choice over 24 months following the implementation of the new default. To do so, we restrict the set of default plans we allow beneficiaries to be assigned to in year t to only those that do not lose benchmark status in $t+1$. Specifically, for beneficiaries in plans losing benchmark status from year $t-1$ to t, we restrict to those assigned to plans that retained benchmark status in $t+1$, We implement this restriction based on the beneficiary's assigned plan rather than actual enrolled plan, as the assigned plan was randomly selected. Likewise, for the control group of beneficiaries in plans not losing benchmark status from year $t-1$ to t, we restrict to those whose plan of enrollment as of December of year $t-1$ retains benchmark status in both t and $t+1$. After these restrictions, we are left with 304,602 beneficiary-experiment records in our restricted sample.

We replicate our difference-in-differences analyses with this new sample. Figures A27 and A28 present event study estimates of the effects of the change in default on drug spending, with Figure A27 presenting overall effects and Figure A28 presenting effects stratified by plan fit. These results indicate that the spending reductions from the change in the default persist through the end of the second year after the new default was implemented. Further, they show that the difference in the consequences of being assigned to a high-fitting versus a low-fitting plan are also persistent, implying that the consumption losses are permanent and beneficiaries do not offset them by eventually taking up different drugs that do appear on the new plan's formulary or by switching to a plan with a smaller effect on drug spending.

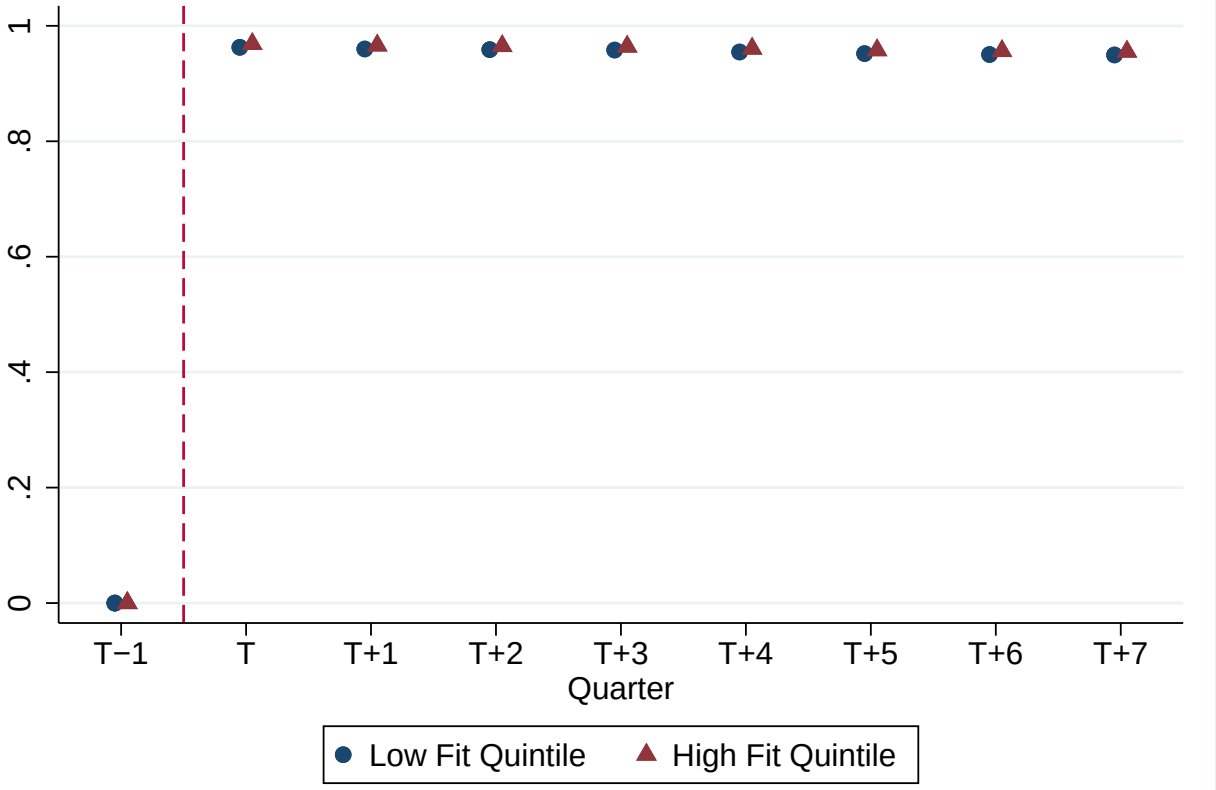
Figures A29 and A30 present event study estimates of the effects of the change in default on the probability of switching plans and the probability of making an active choice, respectively. Again, the effects are persistent. Beneficiaries who faced a change in default switched out of their incumbent plans at extremely high rates and did not switch back within 24 months of the implementation of the new default. They also were not induced by the change in default to make an active choice. The rate of active choice was extremely low after the change in default and did not increase over time. Further, there is effectively no difference in effects on switching or active choice for beneficiaries assigned to higher- versus lower-fitting plans. Again, these results imply that beneficiaries did not respond to the negative effects of the new default by switching plans or making active choices later. Instead, they just passively followed the new defaults and absorbed the persistent reductions in drug spending.



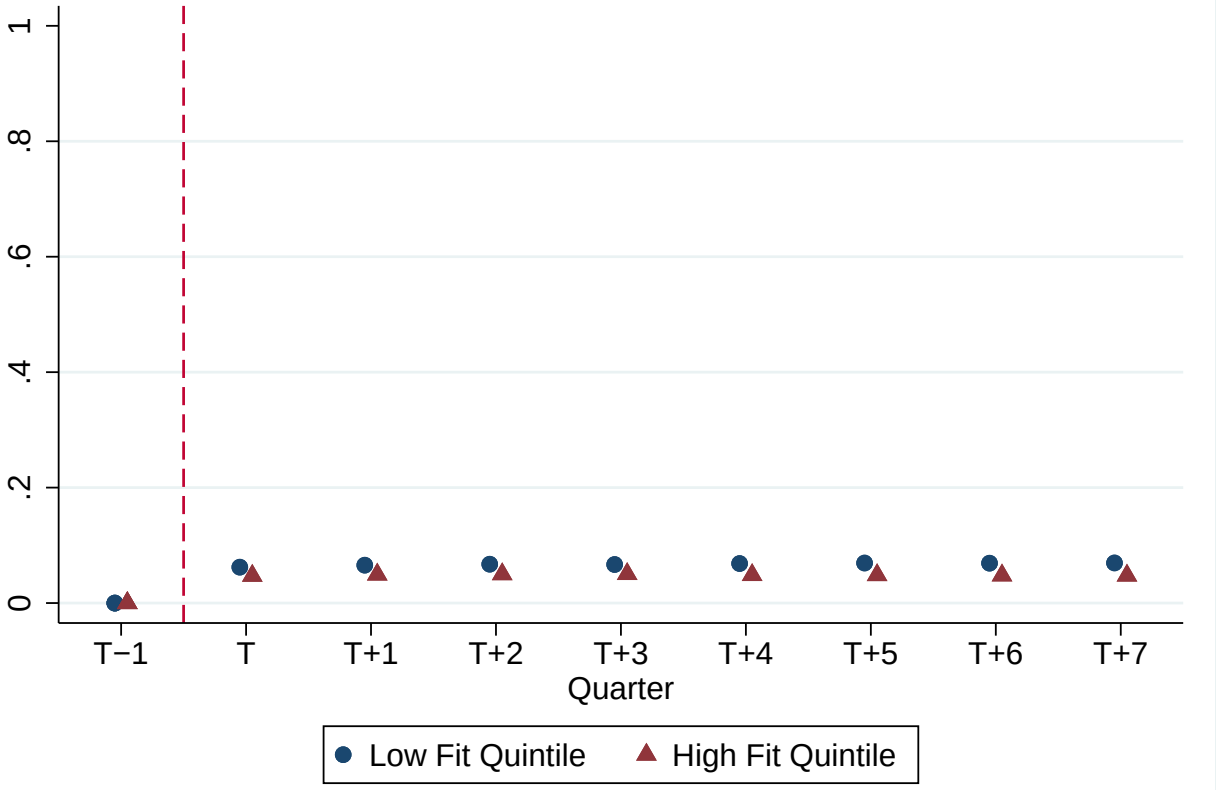
Appendix Figure A27: This figure plots event-study regression estimates of the effect of reassignment to a new plan on quarterly drug spending. This is a replication of Figure 5a with our two-year follow-up sample.



Appendix Figure A28: This figure plots event-study regression estimates of the effect of reassignment to a new plan on quarterly drug spending, for beneficiaries assigned to plans of differing 'fit.' This is a replication of Figure 5b with our two-year follow-up sample.



Appendix Figure A29: This figure plots the propensity of beneficiaries whose incumbent plan lost benchmark status to switch from the plan they were enrolled in during December of year $t-1$, stratified by the fit of the plan they were randomly assigned as a default for year t, relative to beneficiaries whose plan did not lose benchmark status. This is a replication of Figure A14 with our two-year follow-up sample.



Appendix Figure A30: This figure plots the propensity of beneficiaries whose incumbent plan lost benchmark status to make an active plan choice (not entering the default mechanism *or* switching plans after reassignment), stratified by the fit of the plan they were randomly assigned as a default for year t, relative to beneficiaries whose plan did not lose benchmark status. This is a replication of Figure 6 with our two-year follow-up sample.

D Appendix: Further Model Results

In Section 5, we describe a general framework for thinking about default effects manifested through a model of inattentive choice. In this Appendix, we expand on some implications of that framework: How to think about different nested models, as well as welfare and policy implications.

D.1 Example Parameterizations of $A(\cdot)$

First, we can build intuition by considering some common parameterizations of $A(\cdot)$, the latent attention function. We consider a set of parameterizations of the form $A(v^*, v^d, c; \theta) = \beta(v^* - v^d) - c + k$. Under such parameterizations, the agent trades off the benefit of making an active choice, $(v^* - v^d)$, against the cost of doing so, c, putting some relative weight β on the benefits versus the costs. They also face random welfare-irrelevant (in the sense of Bernheim and Rangel (2009)) transitory shocks to attention, k, drawn from a distribution $F(\theta)$. We assume that v, c, and k are denominated in the same units.

In Figure A31 we plot the probability of making an active choice (which we refer to as $a(\cdot)$), as a function of the value of the default plan option v^d. This can be thought of as the 'stakes' at play in making an active choice. When the default is better for the agent, they have less to lose by not making a choice. We consider three specific parameterizations that are exemplary of common models used in the literature and discuss each in turn. While we refer to these models as representing 'attention,' what we describe as 'paying attention' can also be thought of as consideration, or overcoming hassle costs, or any other costly decision effort.

Rational Inattention. First, in dark blue, we plot choice as a function of the value gap for a fully rational agent for whom $\beta = 1$, $k = 0$ always, and $c > 0$ is a constant. In such a model, the agent *never* makes an active choice if $v^* - v^d - c < 0$, and *always* does so if $v^* - v^d - c \geq 0$. Since the welfare gains to making an active choice are $v^* - v^d - c$, we describe this parameterization as representing a 'rationally attentive' chooser, who only makes an active choice when the expected benefit is greater than the cognitive cost. This is the model used by Handel (2013), and is the general workhorse model in the literature on active choice in health insurance and other markets for contracts. Models of rational search (Honka 2014) and rational inattention (Gabaix 2014, Matějka and McKay 2015, Brown and Jeon 2022) follow a similar active choice rule, in which agents rationally trade off costs and benefits, albeit replacing v^* with the expected v^j received as a result of searching or being attentive given the agent's prior information and c with the expected total cognitive cost they will incur.

Boundedly-Rational Inattention. Second, in green, we plot the active choice probability as a function of the value gap for an agent for whom, again, c is constant and $k = 0$, but now, $0 < \beta < 1$. The difference between this model and the rational inattention model is obvious: The curves have the same shape, but the new agent always makes an active choice if $\beta(v^* - v^d) - c \geq 0$, i.e. if $v^* - \frac{c}{\beta} \geq v^d$. This agent is 'boundedly-rationally attentive' in the sense that she understands the incentives facing her and responds correctly in a qualitative sense, but still sometimes makes mistakes in deciding when to pay attention and when not to. Here, the specific mistake is excessively weighting the decision cost over the

expected benefit of active choice. This differential weighting causes agents to fail to make an active choice when $v^d \in \left(v^* - \frac{c}{\beta}, v^* - c\right)$, situations in which a rationally-attentive agent would make an active choice. This is a mistake because the agent would receive a positive net welfare improvement from making a choice in this range. A special case of this parameterization is the beta-delta model of present-bias most commonly used in the literature on default effects in retirement saving (Choi et al. 2003, Carroll et al. 2009, DellaVigna 2018), if we think of agents as discounting their *future* value of insurance coverage relative to their *current* cost of making an active choice. The fact that mistakes are made on an intermediate interval is consistent with the logic in these models that agents procrastinate when the stakes are sufficiently low, but do not do so when stakes are very high. This specific parameterization does not reflect the various ways in which agents can be boundedly-rationally inattentive, but common alternatives have similar features, typically involving the agent shading or being unaware of some part of the benefit of their choices (Ho et al. 2017, Heiss et al. 2021).

Random/Exogenous Attention. Finally, in brown, we plot the active choice probability for an agent for whom c is once again a constant, but for whom $\beta = 0$ and $k \sim U[c - (1-\theta), c + \theta]$ for $\theta \in (0, 1)$. That is, $(100 \times \theta)\%$ of the time, the agent receives a transitory positive attention shock that is big enough to induce them to overcome their decision costs. For such agents, attention is entirely driven by transitory shocks, and not by any fundamentals in the value of their available options. We think of these types of agents as being irrationally or exogenously inattentive in the sense that their active decision-making is completely orthogonal to the benefit of making an active choice. The literature on inattention and salience in taxation (Chetty et al. 2009) has implicitly relied on models similar to this parameterization, in that they model agents as having some fixed probability of, e.g. ignoring sales taxes.[34]

The decision rules embedded in these models are simple, but they highlight the similarities and differences between these views of active choice. While there are important distinctions between rational and boundedly-rational models in terms of welfare considerations, Figure A31 shows that typical boundedly-rational models, in terms of unequal weights on decision criteria, still are very 'rational' in the sense that paying attention is still increasing in the benefit of doing so.

D.2 Welfare

In Section 5.2, we discuss the idea that the attention elasticity governs the extent to which welfare losses from behavioral biases are bounded. In Figure A32 we illustrate this graphically, by plotting the relationship between value of the default, v^d, and expected welfare, under the welfare measure given in Section 5.2, for the three parameterizations we describe above. The dark blue line once again represents the rational inattention case, with $\beta = 1$ and $k = 0$. As shown in A31, this agent does not make an active choice when the potential welfare loss is less than c, $v^* - c < v^d$. In this range, the agent's expected welfare is equal to v^d. When the gap between v^* and v^d exceeds the cost of making an active choice c, however, this agent rationally makes an active choice and receives $v^* - c$ instead of v^d. Importantly, the expected welfare is

[34] As Morrison and Taubinsky (forthcoming) point out, one can think of the parameters estimated in this way as reduced-form representations of an underlying model that looks more like a rational inattention model.

equal to $v^* - c$ no matter how big the potential welfare loss gets because once the gap between v^* and v^d exceeds c and triggers the agent to make an active choice, the value of the default no longer matters.

The green line represents the 'boundedly-rational' case, with $k = 0$ as before but now with $0 < \beta < 1$. As in Figure A31, this agent mimics the fully rational agent when the potential welfare loss is less than c, $v^* - c < v^d$. However, this agent continues to neglect to make an active choice beyond that range. Instead, this agent continues to be passive until $v^d = v^* - \frac{c}{\beta}$. This occurs because $0 < \beta < 1$ causes this agent to over-value the cost c of making a choice today relative to the benefit of that choice tomorrow, $v^* - v^d$. This agent thus *mistakenly* fails to make an active choice when $v^d \in (v^* - \frac{c}{\beta}, v^* - c)$. Figure A32 illustrates why this is a mistake: The boundedly-rational agent experiences a greater welfare loss than the rational agent would have if faced with the same decision. When the v^d exceeds $v^* - \frac{c}{\beta}$, however, this agent makes active choices and receives payoff $v^* - c$, as the rational agent does. This figure illustrates the idea highlighted in Carroll et al. (2009) — such an agent may be worse off with a relatively more generous default (such that $v^d \in (v^* - \frac{c}{\beta}, v^* - c)$), than with a harsher default (such that $v^d < v^* - \frac{c}{\beta}$).

The brown line represents the 'random attention' case, where $\beta = 1$ but each agent draws k from a distribution $U[c - (1 - \theta), c + \theta]$. Here each agent pays attention with probability $p = \theta$. Under this model, the expected welfare is just equal to $W = \theta(v^* - c) + (1 - \theta)v^d$. No matter what the default is, a random $(100 \times \theta)\%$ of agents make active choices and receive according payoffs, with the residual passively accepting the default payoff.

Figure A32 illustrates that, under both the rational and boundedly-rational models, the welfare losses from agent misallocation have a strict upper bound, and therefore welfare has a strict lower bound even as the default gets worse. In the rational model, this loss is bounded by the cognitive cost c. This is a direct consequence of the 'rationality' at play: A rational agent will never allow themselves to suffer from a sufficiently poor state of the world that can be fixed with (costly) action. Models that allow for bounded rationality in the active choice decision relax these welfare bounds, though a bound still exists—in the present-bias model, the bound is instead $\frac{c}{\beta}$. Again, boundedly-rational agents will not allow themselves to suffer too much, although in this case they will downweight the consequences of poor plan fit relative to the costliness of reparative action. In contrast, the random attention model *does not have such a bound*. Agents' expected welfare is strictly monotonic in v^d. This arises from the fact that this agent does not respond to the state of the world at all and thus in many circumstances will simply take what comes. This important difference between the models of active choice will play a critical role in determining how optimal defaults differ across models.

D.3 Optimal Default Policy

Given this discussion of welfare, we can discuss how to optimally assign v^d to maximize expected welfare. We focus on a conflict highlighted in Carroll et al. (2009), Bernheim et al. (2015), and Goldin and Reck (2022). They point out that under various normative assumptions, optimal defaults can cut one of two ways. On the one hand, it may be optimal to design default policy to maximize v^d, to minimize opt-outs and 'nudge' agents into their best plans through smart defaults (Thaler and Sunstein 2003, Handel and Kolstad 2015a). On the other hand, it may be optimal to design default policy to *minimize* v^d, to *maximize* active

choice, thus ensuring agents end up in their most-preferred plans.

We can set this question up akin to an optimal taxation problem by taking a first-order condition with respect to v^d:

$$\frac{\partial W}{\partial v^d} = \underbrace{1 - a(v^*, v^d, c)}_{\substack{\text{Benefit to} \\ \text{inframarginal} \\ \text{passive agents}}} + \underbrace{\frac{\partial a(v^*, v^d, c)}{\partial v^d}}_{\substack{\text{Marginal agents} \\ \text{induced to not make} \\ \text{an active choice}}} \underbrace{E[v^* - v^d - c | A(v^*, v^d, c) = 0]}_{\text{Value of choice for marginal agents}}$$

As we describe in Section 5.2, this highlights the trade-off of changes to v^d. Raising it, by attempting to better suit defaults to agents, is helpful for inframarginal passive agents, whose allocation improves. However, it may damage a set of marginal agents, for whom the default improvement induces them to stop paying attention and follow the default; if the default is worse than what they would have chosen otherwise, this reduces their welfare.

Figure A33 illustrates these effects of a marginal decrease of $\Delta v^d < c - \frac{c}{\beta}$ in the value of the default for the boundedly rational case. As in Figure A32, the value of the default *under the initial default* v^d is on the x-axis and the expected welfare W is on the y-axis. The green line represents the case of the initial default, and the magenta line represents the case of the new, lower-value default. It is straightforward to see that the new, lower-value default results in a welfare loss for the set of agents who fail to make an active choice under either the initial default or the new default, those for whom $v^* - v^d < \frac{c}{\beta} + \Delta v^d$. These agents follow the default in both cases, so the lower defaults obviously lowers their welfare, with this welfare loss illustrated by the area shaded in light red. At the same time, there is a welfare gain for agents for whom the lower default pushes their default value low enough to induce them to make an active choice. These are those for whom the value of the default was previously between $v^* - \frac{c}{\beta}$ and $v^* - \frac{c}{\beta} + \Delta v^d$. They were previously mistakenly making an active choice, since $v^* - c > v^d$, and

These agents have potential welfare loss in the range $\frac{c}{\beta} + \Delta v^d < v^* - v^d < \frac{c}{\beta}$. The welfare gain they experience due to the decrease in the value of the default is illustrated by the area shaded in light blue. When assessing whether a lower-value default harms or improves overall social welfare, the size of the light pink area must be compared to the size of the light blue area. Depending on the primitives, either a lower-value default or a higher-value default could be optimal.

The intuition for the result that a lower-value default could be socially beneficial is that if the policymaker is able to set a default with sufficiently low value, she can 'shock' all agents into making an active choice and receive $v^* - c$ in welfare. But if the policymaker does not go far enough, some inframarginal passive agents will suffer due to the decrease in the value of the default plan, in which they will ultimately enroll. In such a trade-off environment, the planner must generally decide between two extremes: A 'benevolent' default, which provides the maximum achievable default value, or a 'shocking' default, which provides $v^* - c$, although this latter piece assumes that there is a feasible default that is sufficiently punishing to induce universal active choice.[35]

[35]The planner's ability to set default values to be very high or very low is likely to be constrained. Information asymmetries (i.e., not knowing the preferences of agents) restrict a planner's ability to match agents to their optimal plans. Political constraints may limit the planner's ability to set harsh defaults–for example, taxing passive agents is unlikely to be an acceptable policy. The 'shocking' policy studied in Carroll et al. (2009) merely changed the *framing* of the consequences of a failure to make an active

Note, however, that the existence of this trade-off, specifically the existence of the negative (second) effect, depends critically on two conditions. First, it requires $E[v^* - v^d - c|A(v^*, v^d, c) = 0] > 0$, which is equivalent to the idea that, on the margin, agents who fail to make an active choice are experiencing a welfare loss. While this seems like a trivial point, we note that it is *not* true for rationally-inattentive agents. For an agent with the 'rational inattention' parameterization $A(v^*, v^d, c) = v^* - v^d - c$, note that

$$E[v^* - v^d - c|A(v^*, v^d, c) = 0] = E[v^* - v^d - c|v^* - v^d - c = 0] = 0$$

and therefore there is no trade-off in the optimal default policy and thus no benefit to a shocking default. More boundedly-rational models of decision costs do, however, induce 'mistakes at the margin.' For example, the 'mistake on the margin' in the present-biased parameterization above is equal to $E[v^* - v^d - c|\beta(v^* - v^d) - c = 0] = \frac{1-\beta}{\beta}c > 0$ for *any* value of v^*, v^d.

The existence of these mistakes at the margin is a necessary condition for the negative effect in Equation (2) to be non-zero, but it is not sufficient. The second necessary condition is that $\frac{\partial a(v^*, v^d, c)}{\partial v^d} < 0$, i.e. that increases in the value of the default option induce some agents to become passive. In other words, if there are no agents on the margin of making an active choice or not, it does not matter for policy whether the (non-existent) marginal agents are making mistakes. While it is likely (though not guaranteed) that there will be marginal agents when attention is rational, it need not be true more generally. When $\beta = 0$ in our parameterization above (e.g. in the 'random attention' case), $\frac{\partial a(v^*, v^d, c)}{\partial v^d} = 0$ *always* and thus there is no trade-off.

Generating optimal default policy with no trade-off is clear: When there is no harm to marginal agents, improved defaults benefit inframarginal passive agents at no cost to the marginals, and thus the optimal default should be to minimize opt-outs by maximizing v^d. On the other hand, when there is a trade-off, it may be optimal to ratchet down the value of the default.[36] Prior work, when evaluating this trade-off, has focused primarily on considering the extent to which agents are making a mistake (on the margin and otherwise) when they do not make an active choice. This is an inherently *normative* question. Bernheim et al. (2015) and Goldin and Reck (2022) show that different normative assumptions about whether choice passivity is a 'mistake' or a response to real decision costs can generate both of the two extreme optimal default policies we describe above. While having a specific model of behavior is important, our exercise in this section suggests that simply making an assumption about the extent of the mistake is sufficient for optimal policy even without a fully-specified model.

More importantly, this prior work has restricted itself to model domains in which $\frac{\partial a(v^*, v^d, c)}{\partial v^d} < 0$ by assumption.[37] If this is not true, and agents do not adjust their passivity (or make only very minor adjust-

choice rather than imposing any additional material consequences.

[36] Additional considerations include the extent to which the policymaker can achieve a v^d close to v^* by correctly picking a beneficiary's welfare-maximizing plan option; if they can get very close, a 'paternalistic' default may be optimal even in the presence of a trade-off, because at such high values of v^d, if $c > 0$, then $E[v^* - v^d - c|A(v^*, v^d, c) = 0] < 0$. When this is not true, and the policymaker would prefer to ratchet down the value of the default, extreme 'shocking' defaults can arise as an optimum because the share of inframarginal agents who are hurt by such a default decreases as the default becomes more harmful and agents become 'choosers,' and thus $\frac{\partial W}{\partial v^d}$ may *grow* in magnitude as v^d falls.

[37] Goldin and Reck (2022) specifically employ a model akin to our 'rational inattention' model with $A(\cdot) = v^* - v^d - c$, but allow c to have welfare-relevant and welfare-irrelevant components. The size of the welfare-irrelevant component of c in their model is equivalent to the size of what we call the 'mistake on the margin.'

ments) in response to differences in the value of the default option, *then the question of whether or not their passivity is a 'mistake' is moot*. Our analysis in Section 6 evaluates whether this model domain is the correct one to theorize within.

D.4 Heterogeneity in c

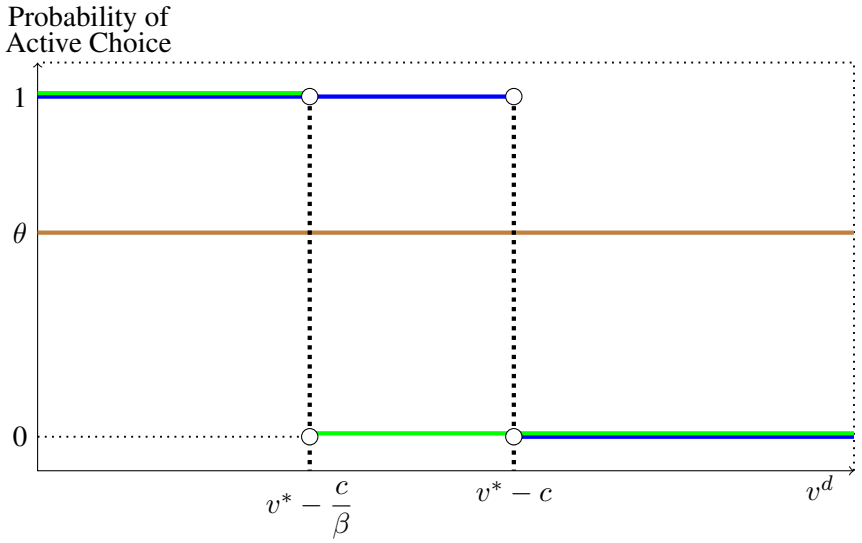
Figure A31 presents the relationship between active choice propensities and default value for a single representative agent with a fixed cognitive cost c. However, when we take this model to data, it need not be true that c is constant across the population. Take, for example, the case where $c \sim U[\bar{c} - \epsilon, \bar{c} + \epsilon]$. In Figure A34 we consider how, if we hold the domain of analysis with respect to v^d constant while widening the distribution of c, how the relationship between v^d and the probability of active choice will appear. We can see in that figure that, for increasingly wider distributions of c, the observed relationship flattens out.

This is important as we attempt to empirically quantify the attention elasticity to v^d. If the distribution of c is wide relative to observable variation in v^d, then we will estimate a flat relationship between the two even under rational inattention. This is an important motivator of our analysis in Section 4 quantifying the extent of effects of default changes.

Given that the distribution of c is unknown, what can we conclude from observation about active choice responses to default stakes? Consider a setting where v^* is constant and c is randomly distributed with cumulative density function F, and assume the econometrician observes two distinct exogenously-assigned defaults, v_0^d and v_1^d, with $v_0^d < v_1^d$. Moreover, assume $A(\cdot) = v^* - v^d - c$, i.e., the rational inattention case. For each default D, agents will make an active choice if $c < v^* - v_D^d$, and the share who do so will thus be given by $F(v^* - v_D^d)$. If the econometrician runs a regression of active choice status on D, the estimated coefficient will reflect the share of beneficiaries who are 'marginal' within that range. They can only be marginal if $c \in (v^* - v_1^d, v^* - v_0^d)$. If c is greater, they will not make an active choice under either default assignment; if c is less, they will *always* make an active choice. Therefore, the estimated coefficient Δ will reflect $F(v^* - v_0^d) - F(v^* - v_1^d)$, the density of agents with c within the relevant range.

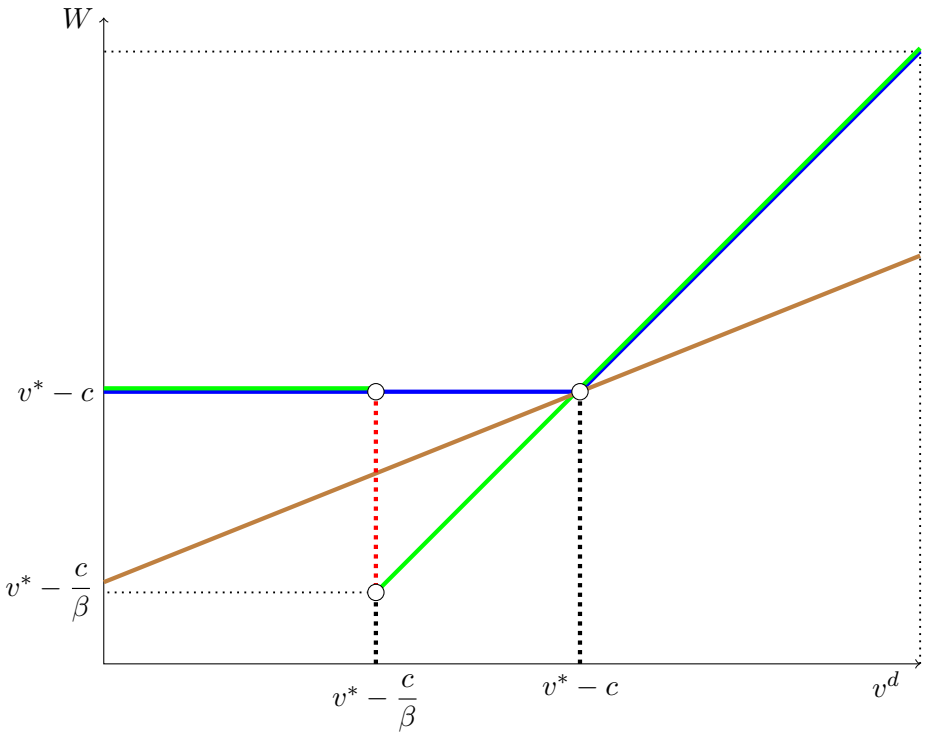
Therefore, for any estimate of Δ, we can rule out any distribution of c with density Δ in the range $(v^* - v_1^d, v^* - v_0^d)$. To the extent that observed default assignments have larger variation in value v^d, we can rule out density in a greater domain.

Appendix Figure A31: Theoretical Relationship Between Default Value and Active Choice



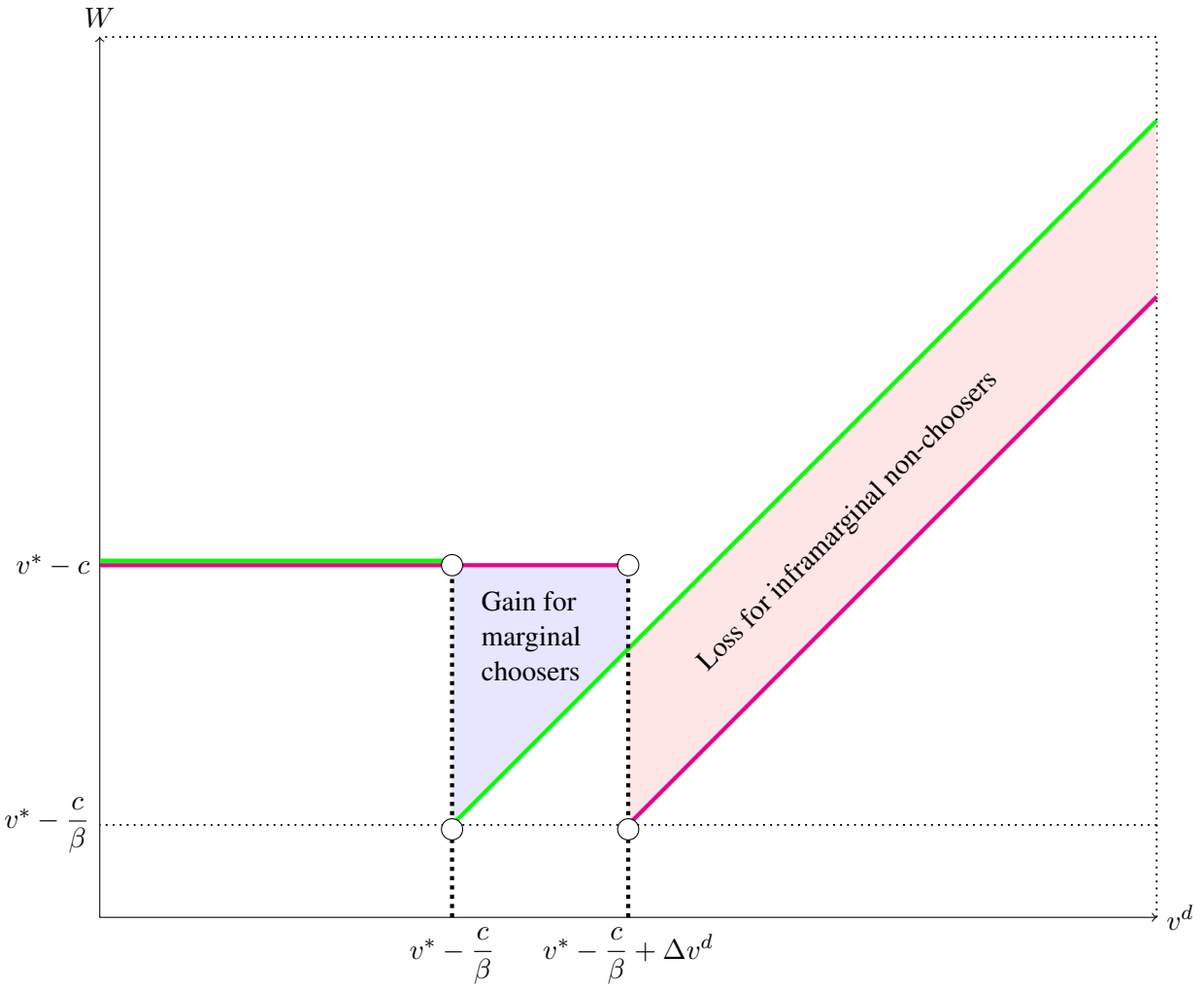
Notes: This figure plots the relationship between the value of the default v^d and the ex ante probability of making an active choice under various models of active decision-making. The blue line represents the 'rational inattention' case where beneficiaries put equal weight on the costs of making an active choice and the expected benefits of doing so, $\beta = 1$ and $k = 0$. The green line represents the 'boundedly-rational inattention' case where beneficiaries 'over-weight' costs relative to benefits, with $0 < \beta < 1$ and $k = 0$. The brown line represents the 'random inattention' case where beneficiaries randomly make active choices, with $\beta = c = 0$ and $k \sim U[c - (1 - \theta), c + \theta]$.

Appendix Figure A32: Theoretical Relationship Between Default Value and Welfare

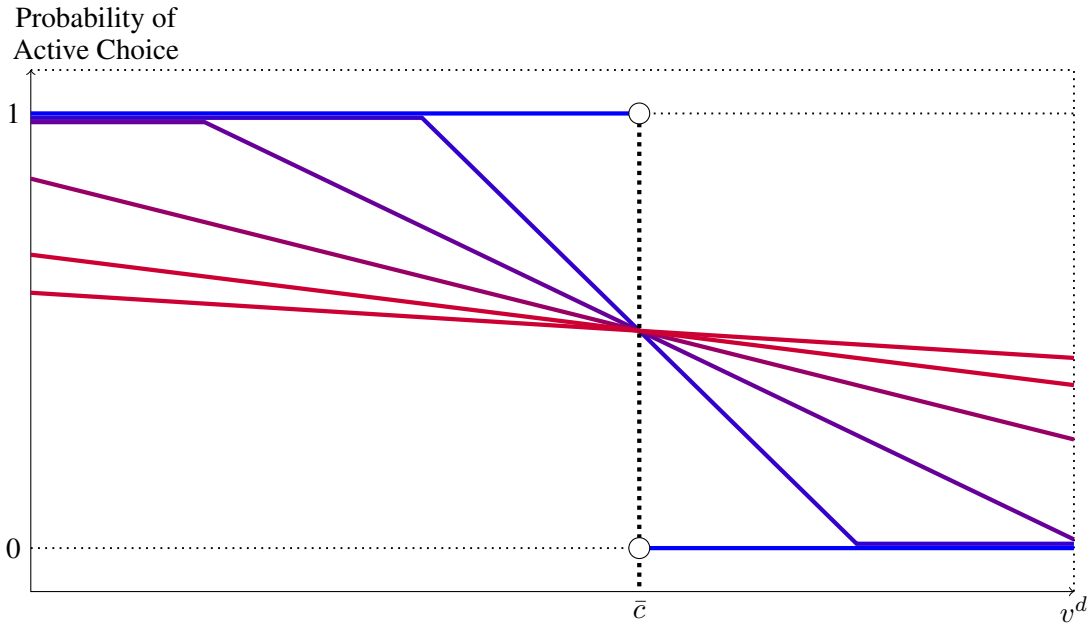


Notes: his figure plots the relationship between the value of the default vd against expected welfare (taking into account the probability of making an active choice) under various models of active decision-making. The blue line represents the 'rational inattention' case where beneficiaries put equal weight on the costs of making an active choice and the expected benefits of doing so, $\beta = 1$ and $k = 0$. The green line represents the 'boundedly-rational inattention' case where beneficiaries 'over-weight' costs relative to benefits, with $0 < \beta < 1$ and $k = 0$. The brown line represents the 'random inattention' case where beneficiaries randomly make active choices, with $\beta = c = 0$ and $k \sim U[c - (1 - \theta), c + \theta]$.

Appendix Figure A33: Welfare Effects of Default Changes



Notes: This figure plots the relationship between the value of the default v^d against expected welfare (taking into account the probability of making an active choice) for a boundedly rational consumer with $0 < \beta < 1$ and $k = 0$. The magenta line plots the same relation when v^d is changed by $\Delta v^d < 0$, with the x-axis representing v^d under the initial default, such that ticks on the axis represent the same individuals across default regimes.



Appendix Figure A34: This figure plots the relationship between the value of the default v^d and the ex ante probability of making an active choice under various distributions of cognitive costs. Each line represents an agent following a model of rational inattention with $\beta = 1$ and $k = 0$, with cognitive costs c distributed randomly in $U[\bar{c} - \epsilon, \bar{c} + \epsilon]$. The blue line represents $\epsilon = 0$, while redder lines represent higher values of ϵ, in $\{0.5\bar{c}, \bar{c}, 2\bar{c}, 3\bar{c}, 4\bar{c}, 8\bar{c}\}$.

E Appendix: Structural Exit Model Derivations

In Section 7, our goal is to decompose the variance in A. Reframing this, we want to estimate $\frac{\text{Var}(c_i)}{\text{Var}(A_{it})}$. Assume that we had panel data where we observed A_{it} directly. Then a simple regression of $A_{it'}$ on A_{it} would recover the variance decomposition. To see this, note that the estimated coefficient would be $\frac{\text{Cov}(A_{it},A_{it'})}{\text{Var}(A_{it})}$. Since k_{it} are i.i.d., $\text{Cov}(A_{it}, A_{it'}) = \text{Var}(c_i)$, and so the estimated coefficient is $\frac{\text{Var}(c_i)}{\text{Var}(A_{it})}$.

However, A_{it} is latent, and instead we observe $a_{it} = 1\{A_{it} \geq 0\}$. The analogue to the above regression, $P[a_{it}|a_{it'}, X]$, does not directly estimate the variance decomposition above. Instead, we must characterize how that object maps onto variances of interest.

To start, note that by construct k is independent across time periods, so $k_{it'}$ is irrelevant for a_{it}. Therefore, the value of $a_{it'}$ in predicting a_{it} is the extent to which it is informative about c. We can start by explicitly characterizing this information by conditioning out c:

$$\begin{aligned} P[a_{it}|a_{it'}, X] &= \int_c P[a_{it}|c, a_{it'}, X] \cdot f(c|a_{it'}, X)dc \\ &= \int_c P[a_{it}|c] \cdot f(c|a_{it'}, X)dc \end{aligned}$$

where the second equality comes from the fact that, by conditioning on c, we have removed any additional information that $a_{it'}$ or X provides, since both are orthogonal to k_{it}, the remaining unexplained component of a_{it}. The second term is the information that $a_{it'}$ provides about the distribution of c given X. This is unknown, but we can decompose it using Bayes' rule:

$$f(c|a_{it'}, X) = \frac{P[a_{it'}|c, X] \cdot f(c|X)}{P[a_{it'}|X]}$$

and therefore, plugging this expression into the initial formula:

$$P[a_{it}|a_{it'}, X] = \frac{1}{P[a_{it'}|X]} \int_c P[a_{it}|c] \cdot P[a_{it'}|c] \cdot f(c|X)dc$$

The term before the integral is the share of beneficiaries with characteristics X who previously made an active choice, which can be computed directly from the data. The term in the integral has two unknowns. First, we need the conditional distribution of a given c. This requires us to make some assumption about the distribution of k. Second, we need the conditional distribution of c given X, which is an unknown that requires a parametric assumption.

In our empirical exercise, we make two distributional assumptions: That $c \sim \mathcal{N}(\mu X, \sigma^2)$, and that $k \sim \mathcal{N}(0, 1)$. The fixed mean and variance of the latter is required as a normalization, as is typical in parametric models of discrete choice. Given these assumptions, $P[a = 1|c] = P[c + k \geq 0] = 1 - \Phi(-c)$ and $P[a = 0|c] = P[c + k < 0] = \Phi(-c)$, while $f(c|X) = \phi\left(\frac{c-\mu X}{\sigma}\right)$.

With these assumptions, we can once again rewrite our conditional distribution, as

$$P[a_{it}|a_{it'}, X] = \frac{1}{P[a_{it'}|X]} \int_c [1-\Phi(-c)]^{a_{it}+a_{it'}} \cdot [\Phi(-c)]^{2-a_{it}-a_{it'}} \cdot \phi\left(\frac{c-\mu X}{\sigma}\right) dc$$

which is the closed-form expression for the likelihood of a_{it} given $a_{it'}$, which can be used in a maximum likelihood estimation procedure to estimate μ, σ^2:

$$\hat{\mu}, \hat{\sigma}^2 = \arg\max_{\mu,\sigma^2} \sum_i \log\left[\int_c [1-\Phi(-c)]^{a_{it}+a_{it'}} \cdot [\Phi(-c)]^{2-a_{it}-a_{it'}} \cdot \phi\left(\frac{c-\mu X}{\sigma}\right) dc\right] - \log\left[P[a_{it'}|X]\right]$$

where the latter term does not depend on μ, σ^2 and therefore drops out.

Note that the integral within this expression has no closed form. We approximate it using Gauss-Hermite quadrature. This entails approximating the integral with

$$\frac{1}{\sqrt{\pi}} \sum_{k=1}^{n} w_k \left[1-\Phi(-\mu-\sqrt{2}\sigma x_k)\right]^{a_{it}+a_{it'}} \cdot \left[\Phi(-\mu-\sqrt{2}\sigma x_k)\right]^{2-a_{it}-a_{it'}}$$

where x_k is are the roots of the Hermite polynomial of degree n, $H_n(x)$, and

$$w_k = \frac{2^{n-1} n! \sqrt{\pi}}{n^2 [H_{n-1}(x_k)]^2}$$

Preliminary tests suggested that this approximation appears to converge at merely $n = 5$ nodes, for reasonable parameter values. For safety, we approximate using $n = 100$ nodes instead. We estimate standard errors in Table 7 using the bootstrap method.